PROCEEDINGS

OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LXX

1931

PHILADELPHIA
THE AMERICAN PHILOSOPHICAL SOCIETY
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OF THE

AMERICAN PHILOSOPHICAL SOCIETY

DURING 1931

Stated Meeting, January 2, 1931

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President in the Chair.

Thomas S. Gates, recently elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following member:

Robert William Rogers, Ph.D., Litt.D., LL.D., S.T.D., at Chadd's Ford, Pa., December 12, 1930, æt. 66.

Vilhjalmur Stefansson, LL.D., read a paper entitled "Travellers' Tales" which was illustrated by lantern slides and discussed by Mr. Bryant.

Stated Meeting, February 6, 1931

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President in the Chair.

The decease was announced of the following members:

James Perrin Smith, A.M., Ph.D., LL.D., at Philadelphia, January 1, 1931, æt. 66.

Joseph Jacques Cesaire Joffre, O.M., G.C.B., at Paris, January 2, 1931, æt. 78.

Richard Bishop Moore, B.S., Sc.D., at Lafayette, Ind., January 20, 1931, æt. 59.

Merkel H. Jacobs, A.M., Ph.D., read a paper on "Diffusion Processes in Non-living and Living Systems" which

was illustrated by lantern slides and discussed by Messrs. McClung, Donaldson, Brubaker and Richards.

Pending nominations were read.

Stated Meeting, March 6, 1931

Albert P. Brubaker, A.M., M.D., LL.D., in the Chair.

The decease was announced of the following member:

Sir Richard Temple, in Switzerland, March 6, 1931, æt. 80.

John Zeleny, Ph.D., read a paper on "The Changes Which Gaseous Ions Undergo With Time" which was illustrated by lantern slides and discussed by Dr. Goodspeed and a guest.

The Committee on Nominations made its report.

General Stated Meeting, April 23, 24, 25, 1931
Thursday Morning, April 23rd
Executive Session—10 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President in the Chair.

Arthur H. Compton, recently elected member, subscribed the Laws and was admitted into the Society.

The President delivered his annual report and appointed the Committees on Nominations and General Meeting.

The following amendment to the Laws was presented and approved by the Society:

In Chapter I, Section I, strike out "At such an election no more members residing within the United States shall be elected than will make the total number of such membership 400" and insert in lieu thereof "Twenty-five members may be elected each year until the total membership of the United States shall be 500 and five foreign members may be elected each year until the total foreign membership shall be 60, these numbers to be kept constant thereafter by electing each year an appropriate number of new members."

The decease was announced of the following members:

Marquis Antonio de Gregorio, in Palermo, Sicily, December 15, 1930.

Leslie W. Miller, A.E.D., LL.D., at Oak Bluffs, Mass. March 7, 1931, æt. 82.

Joseph B. Murdock, Rear-Admiral, U. S. Navy at Danbury, N. H., March 20, 1931, æt. 80.

John Henry Comstock, B.S., at Ithaca, N. Y., March 20, 1931, æt. 82.

President Dercum after having presided during the Executive Session died suddenly while officiating.

Morning Session—10:30 o'clock

William B. Scott, M.A., Ph.D., Sc.D., LL.D., in the Chair.

The following resolution was unanimously carried:

The American Philosophical Society deeply regretting the death of its President resolves that as a mark of respect the annual dinner will not be held this year.

The following Symposium on "The Changing World" was presented:

Section 1. Tendencies in the Natural Sciences

"The Astronomer's Goal," by Frank Schlesinger, Director of the Yale University Observatory. (Read by title.)

"The Assault on Atoms and Molecules," by Arthur H. Compton, Professor of Physics, University of Chicago.

"Hopes in the Biological Sciences," by William Morton Wheeler, Professor of Entomology, Dean Bussey Institute for Research in Applied Biology, Harvard University.

"Lengthening the Span of Life," by Lee K. Frankel, Second Vice-President Metropolitan Life Insurance Company. (Introduced by Dr. Huebner.) Broadcast by the National Broadcasting Company.

"Technology and Material Progress," by Willis R. Whitney, Director Research Laboratory of General Electric Company, Schenectady. (Introduced by Dr. Rice.)

Afternoon Session—2 o'clock

Emory R. Johnson, Litt.M., Ph.D., Sc.D., in the Chair.

Section II. Tendencies in the Field of the Social Sciences

"Economic Adjustment in a Machine Age," by Ernest M. Patterson, Professor and Head of the Department of Economics, University of Pennsylvania. (Introduced by Dr. Johnson.)

"Communications and World Peace," by Waldemar Kaempffert, Science Editor, *The New York Times*. Broadcast by the National Broadcasting Company.

"Unemployment and Its Social Significance," by Arthur Woods, Chairman of President Hoover's Emergency Committee for Employment. (Introduced by President Dercum.) Broadcast by the National Broadcasting Company.

"Thou Shalt Not," by James M. Beck, Member of Congress. Broadcast by the National Broadcasting Company.

Edwin G. Conklin, Ph.D., Sc.D., LL.D., presiding.

Section III. The Changing World

"Round Table Discussion and Recommendations," the following members and guests took part in the discussion: Arthur H. Compton, Henry H. Donaldson, Cyrus Adler, Robert A. Millikan, Edward P. Cheyney, Emory R. Johnson, W. F. G. Swann, Ernest M. Patterson and Charles B. Bazzoni.

Friday Morning, April 24th

Executive Session—10 o'clock

Henry Norris Russell, A.M., Ph.D., D.Sc., Vice-President in the Chair.

The Society proceeded to an election of officers and members.

The Tellers subsequently reported that the following officers and members had been duly elected:

Vice-Presidents

James H. Breasted
Elihu Thomson
Henry N. Russell

Secretaries

Arthur W. Goodspeed
John A. Miller

Curator

Albert P. Brubaker

Treasurer

Eli Kirk Price

Councillors

(To serve for three years)

James M. Beck
Francis G. Benedict
Lafayette B. Mendel
Edwin G. Conklin

Members

Arthur Francis Buddington
Ermine Cowles Case
William Crocker
Raymond Smith Dugan
Alexander Forbes
Simon Henry Gage
Walter Sherman Gifford
Evarts B. Greene
Alfred F. Hess
Leicester Bodine Holland
Earnest A. Hooton
Dugald Caleb Jackson

Frank Billings Kellogg
Carl Otto Lampland
Waldo G. Leland
Howard McClenahan
Wesley Clair Mitchell
Dwight Whitney Morrow
Adolph S. Ochs
John D. Rockefeller, Jr.
Alexander G. Ruthven
J. Henry Scattergood
Herman Augustus Spoehr
Ernest Edward Tyzzer
Willis R. Whitney

Foreign Members

Arthur Stanley Eddington
Arthur Keith

Morning Session—10:30 o'clock

Henry Norris Russell, A.M., Ph.D., D.Sc., Vice-President in the Chair.

The following papers were read:

"A Lost Inner Double Leaf," by Max L. Margolis, Professor of Biblical Philology, Dropsie College for Hebrew and Cognate Learning, Philadelphia. (Read by title.)

"The Archæological Results of the Third Campaign of Tell Beit Mirson," by William F. Albright, Professor of Semitic Languages, Johns Hopkins University. Discussed by Dr. W. B. Scott.

"The Return to Shakespearean Orthodoxy," by Felix E. Schelling, Felix E. Schelling Professor of English Literature, University of Pennsylvania.

"The Temple of Apollo at Bassae," by William B. Dinsmoor, Professor of Architecture, Columbia University. (Introduced by Dr. Barton.)

"Some Economic Aspects of Rome's Early Law," by Tenney Frank, Professor of Latin, Johns Hopkins University.

"The Gottschee Germans of Slovenia," by John Dyneley Prince, Envoy Extraordinary and Minister Plenipotentiary to Yugoslavia. (Read by title.)

Afternoon Session—2 o'clock

Henry H. Donaldson, A.B., Ph.D., Sc.D., in the Chair.

Howard McClenahan, recently elected member, subscribed the Laws and was admitted into the Society.

The following papers were read:

"Untangling One of Nature's Puzzles," by B. Smith Hopkins, Professor of Inorganic Chemistry, University of Illinois.
Dr. Hopkins' paper was broadcast from Chicago by the Columbia Broadcasting System.

"India's Will to be a Nation," by W. Norman Brown, Professor of Sanskrit, University of Pennsylvania. (Introduced by Dr. Adler.)

"John Filson's Map of Kentucky, 1784," by Lawrence Martin, Chief of the Division of Maps, Library of Congress. (Introduced by Dr. Rowe.)

"The Heat Production of the Resting Horse," by Francis G. Benedict, Director of the Nutrition Laboratory, Carnegie Institution of Washington, and Ernest G. Ritzman.

"The Advantages of a Non-luminous Flame for Internal Combustion Engines," by Francis I. duPont, President of the Delaware Chemical Engineering Company.

"The Nervous Regulation of Blood Pressure," by Detlev W. Bronk, Director of the Eldridge Reeves Johnson Foundation for Medical Physics, University of Pennsylvania. (Introduced by Dr. Goodspeed.)

Friday Evening Lecture

Henry Norris Russell, Professor of Astronomy and Director of the Observatory, Princeton University spoke on "The Chemistry of the Stars."

Saturday Morning, April 25th

Morning Session—10 o'clock

HENRY NORRIS RUSSELL, A.M., Ph.D., D.Sc., Vice-President in the Chair.

William Lyon Phelps, recently elected member, subscribed the Laws and was admitted into the Society.

The following papers were read:

"Individual vs. Mass Studies in Child Growth," by Charles B. Davenport, Director of the Department of Genetics, Carnegie Institute of Washington, Cold Spring Harbor. Discussed by Drs. W. B. Scott, Donaldson, Benedict and Parker.

"The Elephant Enamel Method of Measuring Pleistocene Time." Also "Stages in the Succession of Fossil Man and Stone Age Industries," by Henry Fairfield Osborn, American Museum of Natural History and Edwin H. Colbert.

"Origin of the Pebble Band on Iowan Till," by George F. Kay, Dean of the College of Liberal Arts, University of Iowa. (Introduced by Dr. Hobbs.)

"Loess, Pebble Bands, and Boulders from Glacial Outwash of the Greenland Continental Glacier," by William H. Hobbs, Professor of Geology and Director of the Geological Laboratory, University of Michigan. Discussed by Dr. W. B. Scott.

"Early Tertiary Mollusca from Wyoming," by L. S. Russell. Read by William J. Sinclair, Associate Professor and Curator, Princeton University. Discussed by Drs. W. B. Scott and Pilsbry.

"Greenland Expeditions, Actual and Projected," by William H. Hobbs, Professor of Geology and Director of the Geological Laboratory, University of Michigan. Discussed by Drs. W. W. Campbell, Kennelly, Miller, Bryant, Humphreys and W. B. Scott.

"Rationalized vs. Unrationalized Practical Electromagnetic Units," by Arthur E. Kennelly, Professor of Electrical Engineering, Harvard University. Discussed by Dr. Russell.

"The Railroads versus the Weather," by Robert DeC. Ward, Professor of Climatology, Harvard University. (Read by title.)

Afternoon Session—2 o'clock

Albert P. Brubaker, A.M., M.D., LL.D., in the Chair.

Alfred Lee Loomis recently elected member subscribed the Laws and was admitted into the Society.

The following papers were read:

"Preliminary Results on the Application of the Motion Picture Camera to Celestial Photography," by Francis M. McMath, Henry S. Hulbert and Robert R. McMath. Read by Heber D. Curtis, Director of the Detroit Observatory, University of Michigan. Discussed by Drs. Russell, Aitken and Kennelly.

"The Double Stars Showing Orbital Motion," by Robert G. Aitken, Astronomer and Director of the Lick Observatory. Discussed by Drs. Miller and Russell.

"Bearing of Distribution of Phlox Subulata on Glacial Plant Zoning," by Edgar T. Wherry, Associate Professor of Botany, University of Pennsylvania. (Introduced by Dr. True.)

"The Rôle of Cilia and Muscles in the Passage of Sperms and Eggs Through the Mammalian Oviducts," by George H. Parker, Professor of Zoölogy and Director of the Zoölogical Laboratory, Harvard University.

"Osmotic Hemolysis and Zoölogical Classification," by M. H. Jacobs, Professor of General Physiology, University of Pennsylvania. Discussed by Drs. W. B. Scott, Parker and Conklin.

Stated Meeting, November 6, 1931

Edwin G. Conklin, Ph.D., Sc.D., LL.D., in the Chair.

The decease was announced of the following members:

Francis X. Dercum, A.M., M.D., Ph.D., Sc.D., at Philadelphia, April 23, 1931, æt. 74.

Edwin A. Alderman, Ph.B., D.C.L., LL.D., at University, Va., April 29, 1931, æt. 69.

Albert A. Michelson, Ph.D., Sc.D., LL.D., at Chicago, May 9, 1931, æt. 78.

Charles Day, M.E., B.S., E.E., at Philadelphia, May 10, 1931, æt. 52.

George F. Moore, A.M., D.D., LL.D., Litt.D., at Cambridge, Mass., May 16, 1931.

Frank Wigglesworth Clarke, S.B., Sc.D., LL.D., at Washington, May 23, 1931, æt. 84.

Robert Patterson Field, at Philadelphia, June 8, 1931.

Shibasaburo Kitasato, M.D., at Tokyo, Japan, June 13, 1931.

R. A. F. Penrose, Jr., A.M., Ph.D., at Philadelphia, July 31, 1931, æt. 68.

Clifford Herschel Moore, A.B., Ph.D., Litt.D., at Cambridge, Mass., August 31, 1931, æt. 65.

David Starr Jordan, M.S., M.D., Ph.D., LL.D., at Palo Alto, Calif., September 19, 1931, æt. 80.

Dwight Whitney Morrow, A.B., LL.B., LL.D., D.C.L., at Englewood, N. J., October 5, 1931, æt. 58.

Samuel W. Stratton, B.S., D. Eng., D.Sc., LL.D., Ph.D., at Cambridge, Mass., October 18, 1931, æt. 70.

C. A. M. Fennell, Litt.D., at Cambridge, England, January 6, 1916.

Dr. Brubaker moved the following resolution which was unanimously adopted:

RESOLVED That it is with deep sorrow and a keen sense of loss that the Society records the death of its honoured President, Francis X. Dercum.

President Dercum's untiring efforts on behalf of the Society have won the sincere appreciation of his fellow members. He gave his time and energy freely and untiringly to further the interests and advancement of the Society and more especially during the four years of his Presidency he made these interests the chief object of his life's work. His memory will be gratefully and affectionately cherished.

Wilbur W. Swingle, A.B., A.M., Ph.D., read a paper on "The Hormone of the Adrenel Cortex" which was illustrated by lantern slides and discussed by Drs. McClung, Conklin, de Schweinitz and a guest.

The Committee on Nominations nominated Henry Norris Russell to fill the unexpired term in the Presidency.

John Frederick Lewis was nominated from the floor.

Stated Meeting, December 4, 1931

EDWIN G. CONKLIN, Ph.D., Sc.D., LL.D., in the Chair.

James W. Alexander, Charles P. Berkey, William Crocker, Raymond S. Dugan, Leicester B. Holland, Solomon Lefschetz, J. Henry Scattergood and James B. Scott, recently elected members, subscribed the Laws and were admitted into the Society.

The decease was announced of the following member:

Robert DeCourcy Ward, A.M., at Cambridge, Mass., November 12, 1931, æt. 64.

Dr. Goodspeed read an obituary notice of Leslie W. Miller.

George E. de Schweinitz, A.M., M.D., Sc.D., LL.D., read a paper "Concerning Toxic Amblyopias with Especial Reference to Methyl Alcohol" which was illustrated by lantern slides and discussed by Drs. William B. Scott and Emory R. Johnson.

The Society proceeded to an election.

The Tellers subsequently reported that Henry Norris

Russell had been duly elected President to fill the unexpired term.

The minutes of the November Stated Meeting of the Council were submitted and the report of the Committee on Building and Site was approved and its recommendation adopted.

Dr. Brubaker, Curator, read a letter from Mr. Sol Bloom, Associated Director, U. S. Commission for the Celebration of the 200th Anniversary of the Birth of George Washington submitting a request from the United States Government for the loan of the Society's portrait of General George Washington by Gilbert Stuart. After full consideration it was on motion decided not to allow the Washington portrait by Gilbert Stuart to be taken out of the Building.

The following Minute presented by the Council was read:

The American Philosophical Society records its deep sorrow in the loss by death on July 31st, 1931 of its honored member, Dr. R. A. F. Penrose, Jr., whose genuine scholarship and fine personality were for more than twenty-six years an ornament to this Society. He was well acquainted with the plans of the Society for its larger usefulness to science and learning which were under discussion during all the years of his membership, and which had culminated during the past three years in the intensive campaign for increased endowment. His great bequest to the Society comes as a fitting climax to this campaign, and now makes it possible for the Society to realize some of its long-cherished plans for enlarged usefulness. This great endowment imposes upon the Society an equally great responsibility to develop a broad and useful programme that will be a great stimulus to learning and an enduring honor to the name and memory of Dr. Penrose.

The Committee on Finance recommended the following resolution which was approved by the Society:

Resolved That the Treasurer be authorized to transfer from the Principal to the Income Account of the General Fund such amount, as may be necessary to cover any deficit in the operating accounts of the Society at the close of the fiscal year.

The Annual Report of the Girard Trust Company, Trustees of the Building Fund was presented and on motion was referred to the Committee on Audit.

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We are glad to publish this paper by our President, who has now rounded off thirty-nine years of membership and four years in the Presidency, in which office he is still rendering important service.

NON-LIVING AND LIVING MATTER

By FRANCIS X. DERCUM

Prefatory Note: In the within essay the following propositions are maintained.

First: The active factors in "non-living" matter are the same as those present in "living" matter; or, to state the fact conversely, the active factors in "living" matter are the same as those present in "non-living" matter.

Second: The distinction which has been made between "non-living" and "living" matter is artificial.

If physical principles be true not only in a special but also in a general sense, that is, if they constitute a universal truth, they must enter into the explanation not only of the phenomena presented by non-living matter but also into the explanation of living matter. True it is that the difference between a moving animal and its apparently inert surroundings is at first sight great and striking. It is this observation, the outcome of our common daily experience, that has given rise to the fundamental notion so deeply graven in our minds that life is something peculiar; that it is something that differs

from all else; that it is something that is activated by a special and unique principle. Doubtless it is in this and in a host of other kindred and equally primitive experiences that our conception of life has had its origin. However, an increasing knowledge of nature, an increasing knowledge of the structure of the universe, justifies the notion that this conception of life, this interpretation of living things, is after all erroneous. Indeed, the thought arises that the differences which we see and which lead to the distinctions which we make, will upon close analysis disappear. Gradually and with increasing force, the realization comes to us that the distinctions have been made solely by ourselves and have no existence in nature and, finally, that they are wholly artificial.

The inquiry at once occurs, what are the factors that justify this attitude? Clearly we are here referred to questions affecting the fundamental interpretation of nature. The thoughts that first present themselves are those of "matter." Here again, in keeping with our primitive daily experiences, we think of matter as we do of the stones beneath our feet, as something "inert," something "dead," something the opposite of "life." It is this conception unknowingly aroused in our minds by the word "matter" which has led to entirely false notions and has served to accentuate the distinctions with which we have surrounded our conceptions of life. Even in the light of the great discoveries of modern physics, the primitive notions implied by the words "matter" and "material" are so deeply ingrained that they still profoundly influence our attitude of mind, the very trend of our thoughts, and they do so subconsciously. Facts, however, reveal that these conceptions are as erroneous as they are artificial. Of this a brief consideration will convince us. The ultimate structure of matter as originally pictured by the older physicists and chemists was the atom; something that could not be further divided. This atom was not considered as a source of energy but merely as something that responded to the impact of various physical and chemical forces. Of recent times, however, this picture has entirely changed. The atom

has been revealed as made up of multiple constituents; at times small, at times large in number. Primarily it has been resolved into two portions; one a central portion or nucleus and the other a peripheral portion revolving about the nucleus and relatively distant from it. To the nucleus has been assigned the property of a positive electric charge. In the atom of hydrogen, the nucleus has been revealed as a single entity which has been termed a proton, but in the atoms of all the other elements the nucleus has been revealed as being made up of both electro-positive and electro-negative parts but, the electro-positive predominating, the nucleus has always reacted as a positive entity. To the second or peripheral portion the term electron or electrons, according to their number, has been applied. To them has been assigned an electro-negative rôle. The relation between the nucleus and the electrons is comparable in a general sense to the relations which obtain between the sun and the planets of our solar system. These relations are comparable in the same way not only to the motions of the electrons about the nucleus, but to the relative distances of the nucleus and electrons from each other. A convenient illustration is presented by the atom of hydrogen, which is made up of a nucleus (one proton) and one electron, the latter revolving about the former in the course of an ellipse. In a steadily increasing gradation, a mathematical progression, the number of electrons revolving about the nucleus in the various elements has been revealed as progressively larger until finally in uranium ninety-two electrons are reached.

To this interpretation two other factors have been added: one is the "quantum" and the other is the "wave." By the quantum is meant the energy which is given out or received at intervals by an atom. According to Bohr, when given out, it is the result of the "jump" of an electron, as in the instance of hydrogen, from an outer ellipse to an inner ellipse. Such a jump is really a fall of the electron toward the nucleus. The jump results in a given loss of energy and it is this energy which is "radiated." During the ensuing stationary stage,

energy is again absorbed by the electron from the ether waves.

Finally, it is a legitimate inference that the proton and the electron are fast revolving spinning foci of energy; foci of energy revolving in a tenuous medium, a medium that fills all space, that is space. The first, the proton, consists of a positive charge of electricity, the second, the electron, of a negative charge. Thus matter, irrespective of the form in which it may present itself, is an embodiment of energy; so to speak of electrical energy. Assuredly, the modern interpretation of the physicist does away with the primitive notion of matter as being something "inert" or "dead." Assuredly, also, there is apparent now an approach between non-living and "living" matter.

Obviously it becomes important at this point to consider some of the peculiarities of living matter, peculiarities that distinguish or appear to distinguish it from non-living matter. In order to obtain clear conceptions of the problems before us, it is necessary to consider some elementary facts. The form in which living matter, protoplasm, at this period of the world's history presents itself is that of an exceedingly complex substance. The facts show that it is not a chemical compound, but an aggregate of many substances; proteins, carbohydrates, fats and crystalloids or electrolytes. The proteins consist of associations or combinations of many amino-acids; the carbohydrates and fats are diffused or suspended in the general protoplasmic mass. The crystalloids or electrolytes are likewise diffused in solution through the general protoplasmic mass and are resolved into their constituent ions. The physical condition of protoplasm is that of a colloid consisting of many aggregates which play the rôles respectively of continuous and discontinuous phases. At the same time, it is a substance that is undergoing incessant chemical and physical change.

The question as to how this remarkable aggregate originated may never be answered. It is conceivable, of course, that in the unceasing activity of the substances forming the

gaseous, liquid and solid constituents of the planet chemical combinations of increasing complexity would be formed and it requires no illigitimate play of the imagination nor violation of probability to conceive of the combinations of elements leading first to the formation of various forms of hydrocarbons, later to the formation of substances which were perhaps the predecessors of those which we to-day term carbohydrates, and later still combinations in which the elements carbon, hydrogen and oxygen added still another to their number, namely, nitrogen, and thus gave rise to substances which were the forerunners of the amino-acids and the proteins of our own day. Vast periods of time no doubt elapsed before the complexities of living protoplasm were reached. The development not only of the relatively simple oils but of the much more complex fats, the appearance of carbohydrates such as we know them to-day and the associations of large numbers of amino-acids to form proteins, doubtless resulted from very gradual changes. Further, the fact that in protoplasm, fats, carbohydrates and proteins are closely associated is both significant and suggestive.

The origin and the association of the various constituents of protoplasm must for the present at least remain a matter for speculation. According to the view expressed by A. B. Macallum the very earliest organisms must have been ultramicroscopic and they must have synthesized their constituents from the available nitrogen and carbon dioxid in the air, the sulphur from the sulphates, the phosphorus from the phosphates and the iron from the sea water. This view is most suggestive for it leads to the idea that the earliest organisms were bacteria or bacteria-like forms. It is not impossible that the condition of the earth's atmosphere in primeval times were such that little or no light penetrated to the earth's surface or that the dark recesses of the ocean depths offered favorable conditions for bacterial development. In the absence of light, such forms could of course only obtain their carbon in some other way than do the much later appearing chlorophyl containing plants. In fact some of the

known forms of bacteria accomplish this to-day, just as some of them still obtain their nitrogen from the atmosphere though they do so indirectly. Finally it is not impossible that some of the early forms were independent of oxygen and led lives that were purely anaerobic. However that may have been, we are in any event confronted by the basic importance of bacterial life, for upon the bacteria in the earth's surface to-day all other life, plant and animal, is absolutely dependent, and there is assuredly no violation of probability in the thought that bacterial forms were the very first to make their appearance. Bacterial forms, indeed, appear to have been present in rocks now many millions of years old.

The fact already mentioned that living protoplasm is undergoing constant change, a change which in certain phases is very active, insistently demands attention. The problem that naturally presents itself is, what were and what are the factors to be considered? Three unquestionably present themselves: first, the incessant chemical reactions and interchanges; secondly, the play of the catalysts, and finally the dynamic character of the aggregate. These three factors are so closely interrelated that clearness of interpretation forbids their separate consideration. The first may be set aside for the time being with the observation that the chemical changes consist of two processes, one of which results in the upbuilding of substance and the other in the reduction of substance, the two processes being concomitant.

The second factor demands an especial consideration at this time. A catalyst is a substance which by its presence alone sets into activity in other substances chemical processes otherwise dormant without itself undergoing any chemical change. Many instances of such action are seen in the non-living—the inorganic—world; for instance, spongy platinum brings about spontaneously the union of hydrogen and oxygen. Again, by its mere presence it brings about the oxidation of alcoholic vapor. Hydrogen peroxide is decomposed in the presence of soluble alkalies; also in the presence of various insoluble substances such as metallic silver or metallic plat-

inum. In the presence of dilute sulphuric acid starch is converted into glucose; the acid neither qualitatively nor quantitatively undergoes any change. It would appear that in the instance of these "inorganic"—these non-living—catalysts, molecular movement is communicated to the substances which undergo change. Some of the facts are exceedingly suggestive; for instance, in the union of hydrogen and oxygen resulting from the presence of spongy platinum it is the enormous increase of surface in the spongy platinum which is the causative factor of the union. When we turn our attention to protoplasm, we note that this factor of enormous increase of surface is strikingly present. The physical condition of protoplasm is that of a colloid consisting of an enormous number of aggregates which play the rôles respectively of continuous and discontinuous phases. The latter, indeed, can many times be seen in living protoplasm as separate particles suspended in a liquid, for it is to these particles—those of sufficient size to be appreciated under the microscope—that the granular appearance of protoplasm is due. Some idea of the enormous extent of surface thus presented is furnished by the instance of colloidal gold. It is estimated that in a liter of colloidal gold solution of 0.5 gm., in which each particle has a diameter of 15 submicrons, the total surface is equivalent to 65 square meters. We have reason to believe that in protoplasm such factors are vastly exceeded. The structure of protoplasm is such that the surfaces of the discontinuous phases, could they be calculated, would constitute in their totals enormous areas. Obviously the factors that make the surface of spongy platinum so potent are present here and in a degree the magnitude of which it is difficult to conceive.

Living protoplasm has, as instanced in the protozoa as well as in the metazoa, the power of fragmenting various substances. The digestion of a food particle taken into the body of an amœba is an instance of such action. Something is communicated to the food particle, for it finally disappears from observation; it has evidently been so changed as to become

part of the substance of the amœba. Again, instead of building up a more complex body, as in the union of hydrogen and oxygen through the action of spongy platinum, the resulting body or bodies may be more simple, as in the instance of the action of spongy platinum in oxidizing alcoholic vapor. In other words, just as spongy platinum may on the one hand play the rôle of a constructive agent and on the other of a reducing agent, so may living protoplasm play analogous rôles. In addition to a building-up process, such as we note in the digestion and assimilation of a food particle by an amœba, reducing processes also result from the action of the protoplasm. In the higher animals, we know that proteins are reduced successively into peptones, amino-acids and a series of intermediate products until the final stages of urea, uric acid and kindred substances are reached. Similarly, carbohydrates are by a long series of intermediate stages converted into carbonic acid and water. Fats also are decomposed into glycerin and fatty acids until end-products are likewise reached.

In living protoplasm these two processes are always going on: one adding to the protoplasmic aggregate and another simultaneous with the first in which materials are being constantly reduced. In the first, it may be added, energy is being stored up; in the second, energy is being eliminated. Further, these two processes are continuous: the first exceeds the second and thus a constant accumulation of stored-up material occurs. For instance, in the amœba the physical increase in size of the aggregate finally results in a separation or division into two parts, *i.e.*, into two separate individuals. It is this preponderance of the constructive, the anabolic processes, over the reducing, the catabolic processes, which has brought about the wide spread of living matter over the globe. To express the fact in other words, the unceasing chemical reactions in the protoplasmic aggregate and the unceasing catalytic interplay of its constituents result in a storing up of energy, and also in an energy which is being constantly eliminated, the storing up of energy being always

in excess. It is to this excess that the dynamic property of living matter is due.

Finally it may be added that the facts justify the inference that the terms enzymes, ferments and hormones are all expressive of catalytic action and that the term catalysts can be properly applied to them all. Our conceptions are still further widened by the fact that catalysts are not by any means confined to the "organic" world but occur in the "inorganic" world as well. In fact we have here merely a simple extension of physical properties into the domain of "living matter."

Before entering into a more detailed discussion of the properties of living matter let us at this point endeavor to apply the fundamental truths presented by relativity. Assuredly it is not necessary to dwell upon the abstract nature of the conceptions of points, lines and planes, nor of the abstract nature of the conception of the three classical dimensions of space. It is important, however, that we add to the latter the conception of the factor of time. The position of a body may be determined by the measurement of the three classical dimensions, but as every body is in motion and as it requires "time" for a given body to move from one point to another, "time," which is a fourth measurable factor, must be added. It thus becomes the fourth dimension, and we have to deal not with "space" but with the "space-time continuum." All of this is now a twice-told tale but one that demands especial emphasis in our discussion. At once, it becomes evident that the abstract conceptions of the plane geometry of Euclid find here no application. Euclidian geometry is of necessity static, while the conception which the facts of the space-time continuum, as presented by both the "non-living" and the "living" world, force upon us is that of a constantly moving, ceaselessly changing dynamic reality. It would appear that in no object is this expressed with greater truth than in the instance of protoplasm, that moving aggregate the most complex in nature. Perhaps the future will explain how the association of such vastly different substances as hydrocarbons, carbohydrates, proteins, and electrolytes

came about. In the ceaseless chemical and catalytic interplay it is not impossible that the hydrocarbons, carbohydrates and amino-acids grew out of mutual and necessary relations. Indeed, starch, oil and other substances are known to make their spontaneous appearance in the proteins of cells. The constant development of the protoplasmic constituents (their evolution into more complex bodies) and their subsequent reduction (their involution into simpler bodies) is the outcome of their chemical, their physical, their catalytic interplay; and this after all means in its essence an electrical interplay. Finally, the thought is justified that the mutual occurrence of these substances in protoplasm is neither incidental nor accidental, but is a necessary result of factors both pre-existing and concomitant. Further the appearance of the amino-acids and especially their association to form proteins must have introduced at an early stage a dominating catalytic factor, a factor which became especially pronounced when proteins became grouped together and formed nuclear masses. How much these nuclear masses differ from the rest of the protoplasmic aggregate, has been made evident by A. B. Macallum. The nucleus "contains not a trace of potassium or of chlorides, phosphates, carbonates or sulphates, and accordingly no sodium, calcium or magnesium, although these four elements are found in the cytoplasm." Possibly it is in the concentration, the close grouping together of similar given amino-acids, an association in which the alkaline and earthy bases and their salts have no part, that the marvellous dominating power of the nucleus over the cytoplasm has its origin. The nature of this dominating power, whether it be chemical or catalytic or both, becomes a subsidiary question. That it is essentially, if not purely catalytic, is in the highest degree probable. Regarding its extreme importance a moment's reflection will convince us. The pseudopod of an amœba separated from the parent body dies, just as does the nerve fiber separated from its parent cell. Again, in simple cell division, it is the nucleus which plays the essential rôle; without the dominant participation of the nucleus no cell division, no cell multiplication, can take place.

The opposite phenomenon to cell division may be instanced in the conjugation of two cells in which union or fusion of two protoplasmic aggregates takes place. Such a union eventuates in the formation of a new aggregate. The mutual approach of two aggregates before conjugation actually takes place must of course depend upon physical principles. Physical principles have been recognized in the behavior of other aggregates; for instance, in such vastly different structures as the neurones of vertebrates. In the course of development the axones of given neurones definitely approach and finally join given muscle elements. Again, in the course of development, nerve cells are known to migrate. The movement is in the direction from which they habitually receive their stimuli and is clearly expressive of a physical reaction. It has received the name of neurobiotaxis. Kappers believes neurobiotaxis to be an electrical reaction and he has proposed as a preferable term neurogalvanotaxis. There is no reason why a similar physical explanation should not apply to the two aggregates concerned in conjugation. That the approach of the aggregates is purely physical must unhesitatingly be admitted and that it is electrical in character is a justifiable hypothesis. Further, it appears that in given instances the two aggregates concerned are approximately equal in their catalytic power, but should one mass exceed the other in this respect it must obviously become the determining factor in the subsequent changes which ensue chemical, physical, metabolic or whatever we may term them. It would seem that in the course of time such differences in catalytic power once established would in given instances become so great that the catalytic power would be especially emphasized or concentrated in one aggregate and but feebly pronounced in the other. Have we not here a suggestion as to the origin of sex and of sex differentiation? One aggregate, the ovum, is relatively large; the other aggregate, the male element, is exceedingly small. The ovum remains quiescent until conjugation takes place. The spermatozoon consists essentially of a very small, a very minute catalytic aggregate. The impetus conveyed by this

catalytic aggregate is dynamic in its character; for at once when fusion is effected, the most astounding changes ensue. These changes progress by stages—gradual, rapid, prolonged or abridged, continuous or discontinuous—until the previous or "parent" form is reached; or we may say approximated.

Another suggestion offered by a consideration of conjugation is as to the differentiation of plant and animal life. Of two aggregates the one possessing the greater catalytic power would, as pointed out, dominate the second. Under given conditions the second would be so changed as to make it capable of being incorporated with the substance of the first; in other words the second mass would in the process of conjugation become fused with, "digested" and "assimilated" by the first. It is readily conceivable that in the course of time the dominant aggregate would attain a period in its differentiation when it would habitually add to its mass by the appropriation of aggregates of lesser catalytic power and finally that it would itself eventually lose the power of constructing its proteins and other constituents directly from the inorganic materials of the environment. It would then become entirely dependent upon the aggregates which have the sole power of building up their substance directly from the inorganic world; as in the instance of the reaction of light upon chlorophyl. Such an interpretation of the differentiation of plant and animal life appears to be both plausible and probable.

An attempt to pursue this interesting subject in greater detail would be out of place in an essay. The appearance of form, the details of metabolism, the history of the evolution and significance of the internal secretions, and the reactions of organisms to the environment, are subjects so extensive that even a mere outline would take us too far afield. We must content ourselves here with a restatement and summary of a few facts which demand especial emphasis. The first is that the anabolic, the upbuilding, processes in protoplasm far exceed the catabolic, the reducing, processes. As a result protoplasm has undergone an immeasurable increase and in consequence has invaded earth, water and air in every place

where the conditions for its interchanges, chemical, physical or catalytic, exist.

The second fact is that protoplasm is a colloid which is undergoing unceasing chemical and catalytic changes (electrical in character) and that these changes are in necessary relation with and to the environment. Certainly it does not seem strange that this plastic dynamic aggregate with its changing ceaseless internal activities has adapted itself to the most varied environmental conditions. Have we not here an opening key to the problem of evolution?

As pointed out earlier in this essay the conception of the space-time continuum with its four dimensions makes inapplicable the plane geometry of Euclid. Especially in the approach to the living world this fact should be borne in mind. The habit of thought which has been ingrained in us of thinking in terms of Euclidian geometry finds us hopelessly at loss in the problems of the living world. Obviously conceptions of the ever changing, ceaselessly moving space-time continuum are the only ones admissible. Further, our increasing knowledge of the non-living world, especially of the nature and behavior of those seemingly ultimate factors, the protons and electrons, and the realization that protons and electrons are but manifestations of electrical energy, must inevitably and profoundly affect our conceptions of the living world. Distinctions lose their fundamental character and are followed by the recognition of an intrinsic identity.

JURASSIC HISTORY OF NORTH AMERICA: ITS BEARING ON THE DEVELOPMENT OF CONTINENTAL STRUCTURE

By C. H. CRICKMAY

INTRODUCTION

The Problems of Paleogeography. To anyone who has both seen the evidence upon which we base our accounts of geologic history, and accustomed himself to think in terms of the logical interpretations of this evidence, many perplexing difficulties must inevitably bar the way to a clear understanding. Such a one must be puzzled beyond measure at the lack of harmony between the nature of the continents as we now know them and the necessary interpretations of the continents of geologic time. Evidence of former extensions of the continents, evidence of the comings and goings of what have come to be called epeiric and geosynclinal seas, the lack in former times of most of the present day high elevation of the continental masses, lack in those times of the present day heterogeneity in the composition and surface form of the planet. Truly the present is an exceptional age—like none that has passed since the beginning of the Paleozoic. All these things, together with our failure to determine quantitatively the vertical dimension in paleogeography, our failure to know the character of ancient uplands, our wide divergences of interpretation of paleogeography such as the extremes represented by the names of Haug, Suess and Chamberlin, stagger us with a realization of the incompleteness of our understanding.

Plan of Work. These problems have perplexed the mind of the writer ever since he came to know about them. He decided early that a promising plan of attack would be to follow to its utmost limits all the evidence bearing upon the continuous history and paleogeography of a certain region

through a certain short length of geologic time. It seemed to him that such a procedure might permit him, while engaged in the useful pursuit of gathering new information, to find the means of solving some of the persistent problems of paleogeography. For the region he chose North America: for the period, the so-called Jurassic.

Necessity of Limiting the Field. The reasons for the choice seem not to be important except privately: they are therefore suppressed. The reasons for so limiting the field may be explained. It seems unlikely that studies of such broad scope as the great synthesis of world geology brought together by Suess, or the monograph of American paleogeography by Schuchert, will ever be done again successfully by a single person. These works have their place in the development of the science, and now their day is gone. Future workers must strive toward a different ideal—they must delve deeper, they must bring nearer to completion the warp and woof of fine detail. Only so will their observation grasp, and their envisaging reconstruct the true complication of nature. This necessity will compel even the most encyclopædic mind to embrace a narrower field. Probably the main need for such a restriction is the difficulty of keeping abreast of the increase of knowledge of paleontology. Paleogeographical conclusions can not be founded safely except on paleontological correlations. Inaccuracy in the basis or the application spells disaster: Lebling's mistakes in the Paleozoic of the Appalachians are well known.[1] Adequate familiarity with paleontology requires a high degree of specialization—an intensive knowledge of the organisms and the faunas of a limited part of geologic time.

The author has tried to do this; and yet in spite of imposing this reasonable limitation on himself, he realizes more and more as time goes by and the array of detail grows, that with the inevitable subsidiary activities which detract the attention

[1] C. Lebling: *Geol. Rundschau*, 5, 1915. Lebling's work is used as an example because it shows how such mistakes may appear in work of a genuinely high quality. Researches of a lower order positively teem with such errors, as, for example, R. W. Goranson: *American Journal of Science*, Vol. 8, 1924.

for good or ill, one lifetime is a slender resource with which to contend against the formidable, intricate, alas! woefully discontinuous, and yet absorbingly interesting records of even the limited part of geologic history embraced within the Jurassic Period.

THE PURPOSE OF THIS MEMOIR

In proportion to the magnitude of the task the results obtained so far seem hardly sufficient to warrant publication. But in comparison with existing knowledge of Jurassic history, as judged by accounts in textbooks, published summaries, and the like, the results presented in this paper seem somewhat better. Moreover, there is a value in reporting progress of any sort in work of this kind. And further, there is value in bringing together old and new information, and drawing from it a new interpretation. Such a course usually provides for indication of the more urgent local problems in summary form. It may even be a source of encouragement, or at worst a stimulus for argument, to other students.

The persistent problems of paleogeography remain pretty much as the writer found them, but some progress has been made in local studies and in their correlation. North American Jurassic history becomes clearer in many of its details. Improvements have been made in the technique of making paleogeographic maps. Each map represents only the geography of one date. A sufficient number of maps is used to show the culminations of *all* notable geologic events.

Finally, no pretense is made of giving complete stratigraphic details or faunal lists. Only those localities and those particular details which are essential to an argument are used. This work is not a summary of stratigraphy, but rather a sketch of Jurassic history and paleogeography. An attempt is made to reinforce the sketch by reviewing the evidence and the studies which have been made of it, not from the genetic, but from the developmental point of view. The treatment is therefore truly historical.

JURASSIC HISTORY

North America at the Beginning of the Jurassic

The Nucleus or Kratogen. It is generally assumed that at the end of the Triassic, the ancestral North American continent of that time stood well above sea-level. This idea is based mainly on the fact that nowhere on the continent is there any latest Triassic or earliest Jurassic marine sediment. Furthermore, unconformities between Trias and early Jura are reported. This evidence of differential uplift and erosion is assumed, perhaps justifiably, to mean complete uplift above sea-level. These conclusions have plausibility only, not certainty. They may be accepted tentatively, but are no basis on which to build.

The central nucleus of the continent has been called Jurocanadia,[2] but it seems preferable to use a name coördinate with the Suessian nomenclature, hence it is now proposed to call the nucleus of the North American land-mass during the Jurassic Period, Jurolaurentia. The eastern margins of this land are utterly unknown: there is no marine Jurassic in eastern North America. In the north, the nearest older Mesozoic is in the outermost of the Arctic Archipelago. In the south, no deposits of this age are known from the borders of the Gulf of Mexico north of latitude 25°. It would seem as though the north, east, and south boundaries of Jurolaurentia lay far beyond the corresponding coasts of the modern continent. This conclusion is, of course, in ill accord with the fundamental thesis of the permanence of the continents. It seems to be in close agreement, however, with the conclusions drawn by Suess from the running of the western Altaide folds into the Atlantic Ocean.

The Western Margin. In the west, a very different condition is found. The limits of Jurolaurentia lay well within the present boundary of the continental platform. But beyond these western margins, there lay a great zone of land covered with Carboniferous and Triassic sediments and volcanics which extended the continental edge to a position presumably

[2] C. H. Crickmay: Univ. of Calif., Publ. Geol., 19, 1930.

very near the present one. The latter conclusion is based on the distribution of late Triassic and early Jurassic marine deposits in narrow fringes along the present continental margin from Alaska to Central America. This suggests short transgressions of a shelf sea over an initial coastline near the present one. The area flooded by these shelf seas is a peculiar one. It has been interpreted as a geosyncline.[3] Certainly it behaved somewhat like one, subsiding broadly from age to age, and filling in course of time with a vast cumulus of sediment. But though bounded on the east by the continental nucleus, it was bounded on the west only by open ocean. This statement is not in accord with older interpretations which postulate lands such as "Cascadia" along the continental margin. It is now suggested that these older opinions are at fault, in imputing conditions to the Jurassic, simply from analogy with those of other periods. There is no real evidence of the existence, at the beginning of the Jurassic, of lost oceanic land masses such as the conjectural Cascadia, or the "Pacific continent" of Haug. Not only so, but the existence in any period of anything but volcanic land west of the Cordilleran Geosyncline is hardly proved. Such volcanic land may be merely of the nature of oceanic islands. Furthermore, it is now well known that the area of the Cascadia of Schuchert's maps [4] was open sea during much of the Paleozoic. The actual margins of the continental platform are naturally very uncertain. The writer can not help thinking, as he views the somewhat confident interpretations that have been made of rather slender evidence, that most paleogeographers have presumed far too much in this regard.[4, 5, 6, 7] There is no purpose in assuming any of the unproved dicta of this authority or that on a question of such fundamental importance. It is better for the matter to remain very obviously in doubt.

[3] C. Schuchert: G. S. A. Bull., Vol. 34, 1923.
[4] C. Schuchert: Bull. G. S. A., Vol. 20, 1910.
[4] C. Schuchert: *Bull. G. S. A.*, 1910.
[5] E. Haug: *Bull. Soc. Geol.* (3), 28, 1900.
[6] R. Ruedemann: N. Y. State Mus. Bull., 1922.
[7] R. T. Chamberlin: *Jour. Geol.*, 32, 1924.

The Sonoran Geanticline. Within the shelf region, across what is now Sonora, Arizona and Nevada, lay a geanticline. The early Triassic seas had extended broadly from the Pacific eastward to the borders of the continental nucleus. But in the Upper Triassic all this is changed. The geanticline rose and shut the seas out of the Great Basin region. The Upper Triassic sediments of the Great Basin and the Colorado Plateau are of continental character. Moreover, they become coarser, even conglomeratic, toward the south and west, e.g., Shinarump, Chinle, and Timothy formations. The largest cobbles in the Shinarump occur in southeastern Nevada and northwestern Arizona, *i.e.*, the southwestern limits of the formation.[8] So it seems likely that the sediments were derived from the southwest. If this be true, it indicates an upwarping of the Sonoran geanticline, and makes Branson's [9] interpretation of Upper Triassic paleogeography seem doubtful, as Reeside [10] has lately shown. It would seem as though the elevation of this peculiar area persisted into the Jurassic, as Lee [11] has already suggested, for the early Jurassic sediments on both sides of it are coarse. On the east side, the Nugget sandstone, Glen Canyon Series, and equivalents, are coarser in grain, even including conglomerates, to the west. Farther east, they are replaced by much finer sediments. On the west side of the axis, in Esmeralda County, Nevada, the base of the Marine Jurassic series is an angular breccia of chert fragments in a fine matrix.[12] Farther west and north, there are no very coarse sediments earlier than the Middle Jurassic. The Sonoran geanticline was already large, as geanticlines go, at the beginning of the Jurassic; but during that period it extended itself northward in a remarkable manner, finally developing into what has been called the Cordilleran Intermontane Geanticline.

Central Alaska. It is necessary to note another special

[8] C. R. Longwell: Bull. 798, U. S. G. S., 1928.
[9] E. B. Branson: *Jour. Geol.*, 35, 1927.
[10] J. B. Reeside, Jr.: *Jour. Geol.*, 37, 1929.
[11] W. T. Lee: Smithson. Misc. Coll., 64, No. 4, 1918.
[12] H. W. Turner: *Am. Geol.*, 29: 261, 1902.

region in the lower Yukon Valley which was of a different character from the rest of the Pacific border. No Triassic or Jurassic sea ever flooded this area: it was a persistent land-mass. The name Juroberingia is now proposed for it. The region appears to have included what is now Bering Sea, and to have been an outlying province of the Jurassic Asiatic continent.* The existence of this element in Jurassic North America is very important. It shows that the spatial relations of the Asiatic and American continents with one another have not changed greatly since the Jurassic. A thorough substantiation of this by further field study would produce a powerful argument toward a refutation of Wegener's hypothesis of continental drift, or at least of the main claims of the devotees of that hypothesis.

Relief. As far as the evidence goes, it would seem that no part of the continent except the Sonoran geanticline showed any notable relief at the beginning of the Jurassic. The absence of coarse sediments from the borders of Jurolaurentia suggest a low elevation in that quarter. The "ancestral Southern Rocky Mountains" had been reduced to excessively low relief by the beginning of the period.[11] Any relief produced by the Triassic Pacific marginal vulcanism was apparently cut down before the end of the Triassic, because almost everywhere the volcanics are overlain by fine marine sediments of the same period. All along the Pacific Coast the early Jurassic lies upon the late Triassic with little or no discordance. The contact at Parson Bay is typical.[12] Farther from the coast discordances are encountered. In the Interior Plateau of British Columbia, the Middle Jurassic rests unconformably on the Trias.[2] In central Oregon, the Lower Jurassic rests with marked discordance upon the Trias.† In Utah, the basal contact of the Jurassic is an unconformity.[14, 10]

* It is proposed to call this continent Jurangara.

[11] W. T. Lee: Smithson. Misc. Coll., 64, No. 4, 1918.

[12] C. H. Crickmay: Univ. of Calif. Publ. Geol., Vol. 18, 1928.

[2] C. H. Crickmay: Univ. of Calif. Publ. Geol., 19, 1930.

† Mr. R. L. Lupher: Personal communication.

[14] J. B. Reeside, Jr., and James Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.

[10] J. B. Reeside, Jr.: *Jour. Geol.*, 37, 1929.

All these seem to be the result of folding in, or on the borders of, the Sonoran geanticline.

Such was the nature of the platform on which Jurassic history was enacted.

Lower Jurassic Seas and their Faunas

Early Lias of the Southwest. The earliest submergence of the land was caused by a shelf sea advancing from the Pacific. Its earliest record is in Esmeralda County, Western Nevada. From a locality known as Volcano, 30 miles southeast of Walker Lake, Gabb long ago described: "*Ammonites*" *nevadanus* (possibly an *Arnioceras*), *Turbo regius*, *Pholadomya multilineata. P. nevadana, Cardium arcæformis*, *Astarte appressa*, *Pecten acutiplicatus Meek*.[15] These came from a limestone formation. Hyatt's *Vermiceras crossmani* is from this district.[16] This is not a *Vermiceras*, it may belong to *Metophioceras*. If it does, its date would be Coroniceratan, *rotator*. But this is doubtful because of the existence in the West American realm of a homœomorphic series of somewhat different date—*Arniotites*. Hyatt founded the genus *Arniotites*, but he knew very little of its characters, its affinities, or its date of existence. Any ammonites referred by Hyatt or others to *Arnioceras*, *Vermiceras*, *Coroniceras*, or *Arietites* in West America may belong to *Arniotites* which is of later date.

Recently Mr. Siemon Muller has obtained several excellent faunas from this locality. They appear to range from Schlotheimian to Hildoceratan ages. It is hoped that we shall soon see something from Mr. Muller's pen on this subject.

A stratigraphic section in this district taken across the Pilot Mtns. seems to show enormous thickness,[12] unless the published diagram is grossly exaggerated. The basal beds are an angular breccia of chert fragments in a fine matrix. These are overlain by great alternating limestone and shale formations some of which bear Liassic fossils. Then, in order, red shales, cherts, etc. Total thickness, 25,000 feet.

[15] W. M. Gabb: *Am. Jour. Conch.*, 5, 1869.

[16] C. H. Crickmay: Calif. Acad. Sci. (4), 14, 1925.

[12] H. W. Turner: *Am. Geol.*, 29, 1902.

In southern California, the Jurassic record is undiscovered: the Upper Cretaceous rests directly upon the Trias.[17] However, the early Lias occurs in Inyo County, though nothing is known of its geology.[18] In northern California, the Lias is well developed. Mr. S. G. Clark has sent the writer specimens of *Arniotites* sp. from the argillites of the Sailor Canyon formation of Placer County.* Besides this, there are several later Lias deposits in the northern Sierra Nevada which will be noticed in the sequel.

In the California Coast Ranges, the early Lias may possibly be represented by some of the rocks which have been included in the Franciscan Series, though no fossils of that age are yet known in that region. There is no doubt that incongruous elements have been included in the Franciscan by almost everyone who has dealt with it.[19] The older of these foreign elements may well be the earlier Mesozoic of the Coast Ranges. The Franciscan Series will be further discussed in the section on Argovian faunas.

British Columbia. In British Columbia, the two southern records by Daly[20] and by Cairnes[21] seem rather doubtful. However, round the north end of Vancouver Island there are a number of good occurrences.[22] That at Parson[23] Bay, Harbledown Island, is one of the best. Here, the basal Lias consists of 200 feet of greenish quartzite overlain by about 1400 feet of black argillite. The whole is known as the Harbledown formation. It rests disconformably upon the Upper Triassic *Pseudomonotis subcircularis* beds. The formation contains four early Lias faunas.[12] These follow, in descending order:

[17] B. Willis: U. S. G. S., Prof. Paper 71, 1912.

[18] Alpheus Hyatt: G. S. A. Bull., Vol. 5, 1894.

* It will be remembered that these have been regarded as Triassic: Hyatt described *Monotis* and *Daonella* from them.

[19] J. P. Smith: Bull. 72, Calif. State Min. Bureau, 1916.

[20] R. A. Daly: G. S. C., Mem. 38, 1912.

[21] C. E. Cairnes: G. S. C., Mem. 139, 1924.

[22] G. M. Dawson: Ann. Rept., 1886, pt. B, G. S. C., 1887.

[23] J. A. Bancroft: G. S. C., Mem. 23, 1913.

[12] C. H. Crickmay: Univ. of Calif., Publ. Geol., 18, 1928.

"*Gleviceras*" sp.
Melanippites harbledownensis
Arniotites kwakiutlanus
"*Vermiceras*" sp.

From this locality the early Lias sea has been called the Harbledown sea.

Deposits of this age occur on the Queen Charlotte Islands at Houston Stewart Channel, Crescent Inlet, etc.; the latter being the type locality of *Arniotites vancouverensis* Whiteaves.[24, 25] These deposits consist of dark argillites associated with limestone, argillite and volcanics of the Trias. The geology of these localities is only roughly known from Dawson's early explorations.[26]

Alaska. There seems to be some Lias in southern Alaska, but most of it is badly confused with the early Middle Jurassic on the one hand, and the Triassic on the other. Near the head of Nikolai Creek, Chitina Valley, *Arniotites?*, an early Jurassic genus, occurs in the McCarthy black shales part of which are Triassic.[27] In the Cold Bay district on the Alaska Peninsula, there is 2300 feet of sandstone, shale, and limestone, with undescribed faunas of Liassic aspect. From the north shore of Cold Bay itself, there are reported *Arietites?*, *Aegoceras?* and *Amaltheus?* The genera are almost certainly misidentified, but the names could hardly be suggested by forms other than those of the early and mid Lias.[27] Some of the localities on Kenai Peninsula yield Lias faunas. For instance, Point Naskowhak yields *Myophoria?* sp., and a locality two miles west of Seldovia Bay yields *Arietites?* sp. But some of the other localities in this vicinity, so far included in the same formation and called Lower Jurassic, seem more likely to be early Middle Jurassic. These contain volcanics; and it is questionable, if we may judge by analogy with the

[24] J. F. Whiteaves: Appendix 1, Ann. Rept., 1886, pt. B, Geol. Surv. Canada, 1887.
[25] J. F. Whiteaves: Contrib. Can. Pal., Vol. 1, No. 2, 1889.
[26] G. M. Dawson: Rept. of Prog., 1878–79, G. S. C., 1880.
[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.
[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.

rest of the Pacific coast, whether any of the volcanics ascribed to the Lias in Alaska are really much earlier than early Middle Jurassic. Certainly none of the Alaskan volcanics are demonstrably Liassic.

Later Lias Sea in Nevada. The Harbledown sea seems to have continued through most of the Lower Jurassic, possibly retreating in some places and advancing slightly in others. Later Lias deposits occur in western Nevada at "Volcano" or Pilot Mts. from which Gabb listed *Pecten acutiplicatus.*[15] This species is not the true *P. acutiplicatus* Meek which is a latest Lias species from Mt. Jura. It is a somewhat earlier type.

California. Mr. S. G. Clark has sent the writer an undoubted Hildoceratid from the Sailor Canyon Series of Placer County, California. The date should be about Harpoceratan.

The writer has collected a succession of Lower Jurassic faunas from the Hardgrave and related formations at Mt. Jura, California; as follows:

"*Pecten*" *acutiplicatus* Meek from fine red tuff, the typical Hardgrave formation.

"*Pecten*" aff. *acutiplicatus* from grey sandstone, lower part of Hardgrave.

Melanippites? sp. from dark grey, calcareous argillite below the Hardgrave.

The "*Melanippites*" and "*Pecten* aff. *acutiplicatus*" are middle Lias types: the latter, Dumortierian. The faunas occurring with them are small ones, not yet described. The true "*Pecten*" *acutiplicatus* Meek belongs at the top of the Lias. The fauna with which it occurs is a rich one, already listed in outline by Hyatt,[28] and partly described by Meek.[29] The fine sediments in which "*Melanippites*" occurs are typical of the Lias Seas. The coarser ones of the Hardgrave

[15] W. M. Gabb: *Am. Jour. Conch.*, 5, 1869.
[28] A. Hyatt: G. S. A., Bull. 3, 1892.
[29] F. B. Meek: Paleont. Calif., Vol. 1, 1864.

formation mark the close of the Lias and the advent of the Middle Jurassic vulcanism.

Oregon. In central Oregon, Mr. R. L. Lupher is bringing to light some interesting Liassic faunas. They have not been described, but their collector has kindly exhibited them to the writer. They show a broad extension of the middle and later Lias seas in that region. The faunas of the supposed Hardgrave red sandstone of that region have long been known.[18] They are reported to comprise: *Parapecten acutiplicatus* Meek, *Pleuromya concentrica* Meek, *Pholadomya nevadana* Gabb, and *Pholadomya multilineata* Gabb. More recently Hertlein has described "*Uptonia*" *silviesi* from the principal locality, 18 miles north of Burns.[20]

British Columbia. In the Queen Charlotte Islands the work of McLearn [21] has shown the existence of three later Lias faunas; in descending order:

Chlamys carlottensis (Whiteaves)
"*Harpoceras*"—"*Dactylioceras*"
"*Seguenziceras*"

These faunas, which have been found only on Skidegate Inlet, are much later than the *Arniotites* and *Melanippites* faunas of that region. The "*Seguenziceras*" fauna is of Amaltheian age. The "*Harpoceras*"—"*Dactylioceras*" fauna is of Hildoceratan age. And the *Chlamys carlottensis* fauna including "*Pseudogrammoceras*" *propinquum* (Whiteaves) * and "*Cœloceras*" sp. nov. etc., is of Grammoceratan age. These occur in the Maude formation which consists mainly of dark, fine bedded argillites some of which are very calcareous.

General Character of the Lower Jurassic. The evidence of the lithology of the Pacific American Lias—fine, largely calcareous sediments with fine, even, persistent bedding—

[18] A. Hyatt: G. S. A., Bull., Vol. 5, 1894.
[20] L. G. Hertlein: *S. Calif. Acad. Sci.*, 24, 1925.
[21] F. H. McLearn: *Trans. R. S. C.*, XXI, 1927.
* F. H. McLearn's "Notes on some Canadian Mesozoic faunas," *Trans. R. S. C.*, 24, 1930, received after this went to press, puts this species in the "*Harpoceras*" fauna; and gives a much needed generic name, *Fanninoceras*, to the involute, oxycone ammonites of the *Chlamys carlottensis* zone.

suggests a period of diastrophic calm. Even the basal beds are at most places no coarser than fine sandstone. The first coarse beds which interrupt the normal sedimentation are volcanic tuffs of the latest Lias which presage the violent vulcanism of the Middle Jurassic.

The Middle Jurassic Vulcanism

General. Toward the end of Lower Jurassic time, volcanoes appeared along a great belt from southern Alaska, through the Coast Range region of British Columbia, and into California. Great beds of agglomerate and flows of lava piled upon each other on a sinking earth's surface. For one brief spell in the early Middle Jurassic (late Sonninian and early Stepheoceratan) the violent eruptions ceased, and during this interval the seas spread over most of the lava-devastated areas, and left marine deposits. But shortly these were again buried beneath the products of renewed eruptions until thousands of feet of lava and pyroclastics had accumulated.

Alaska. The record of these eruptions is one of the most unmistakable things in west American geology. In Alaska, on Cook Inlet and in the Alaska Range, the Middle Jurassic [32, 33] fossiliferous beds rest on volcanics. In Matanuska Valley [34] and at Windfall Harbour,[35, 37] Admiralty Island, the volcanics contain early Middle Jurassic fossils. At Seldovia Bay the volcanics are largely marine pyroclastics, but on the Alaska Peninsula the Middle Jurassic occurs without volcanics. These last localities show a dwindling and failure of the volcanic products toward the southwest, which gives a clear hint as to the distribution of volcanic centers. In Alaska, as elsewhere, the volcanics are overlain by marine sediments of Sonninian age, indicating a cessation of vulcanism. But most of these Alaskan occurrences are peculiar in that they show no evidence of the second period of vulcanism in later Middle Jurassic time.

[32] T. W. Stanton and G. C. Martin: G. S. A. Bull., Vol. 16, 1905.
[33] A. H. Brooks: U. S. G. S., Prof. Paper 70, 1911.
[34] S. Paige and A. Knopf: U. S. G. S., Bull. 327, 1907.
[35] C. W. Wright: U. S. G. S., Bull. 287, 1906.
[37] G. C. Martin: U. S. G. S., Bull. 776, 1926.

British Columbia. In the Queen Charlotte Islands,[31] 1600 feet of coarse pyroclastics lie upon the Liassic Maude formation which bears faunas as late as Grammoceratan. About the middle of the pyroclastics occur 100 feet of marine sediments with Sonninian faunas. The pyroclastics are overlain by marine sediments with Proplanulitan faunas. A very similar, though thicker, section is found in the Bulkley Valley where the pyroclastic formation is 3000 feet thick. Near the middle of this occurs 500 feet of marine sediments with faunas from Sonninian, *sauzei*, to Stepheoceratan, *Epalxites*. The base of the section is not yet known. The top is made up of sedimentary rocks which have not yet yielded fossils.

In southern British Columbia, volcanics of this age seem to be thicker. In the Harrison Lake district, flows and agglomerates attain the enormous thickness of 9200 feet. Thin marine beds in the lower part of this accumulation yield fossils including an early middle Jurassic species of *Entolium*.*

Pacific States. No comparable volcanics have yet been described from Oregon and Washington. But in northern California the first vulcanism is plainly marked by the discontinuous Bagley andesite, 1000 feet thick, overlying Liassic deposits. This is overlain by the Potem formation the lower part of which consists of sandstone and shale with presumably Middle Jurassic fossils. The upper part is of pyroclastics which record the second vulcanism, that of later Middle Jurassic time.

In the northern Sierra Nevada, at Mt. Jura, the Middle Jurassic succession is mainly volcanics:[36]

Pyroclastics, unnamed, 1000 feet
Mormon sandstone with *Sonninia* sp., 900 feet
Fant meta-andesite flows, 800 feet
Hardgrave red tuff with *Pecten acutiplicatus*, 200 feet

[31] F. H. McLearn: Trans. R. S. C., Vol. xxl, 1927.

* C. H. Crickmay: Field work, 1924, 1926.

[36] Diller's account, U. S. G. S., Bull. 353, 1908, modified by the writer's field work, 1928, 1929.

The fauna of the Hardgrave is late Lias. Ultimately its exact date may be worked out, and thence, the date of the very beginning of the vulcanism.

Mr. S. G. Clark has told the writer that in Placer County, California, there are about 9000 feet of Middle Jurassic volcanics with a few fossils. In Inyo County, there is a great mass of altered, unfossiliferous volcanics.[37] These may be at least in part of Middle Jurassic age.

Conclusions. These various occurrences seem to indicate that the centres of eruption were located in two main belts: one along the site of the present Sierra Nevada, the other along the British Columbia coast and southeastern Alaska. Farther east volcanics are rare and thin, and of very fine grain as though deposited far from a centre of eruption.

The vulcanism had some profound and lasting effects. It broke up the Liassic continental shelf, and formed a massive fringing border of volcanic land, or islands, along the western margin of the continent. The northern part of this fringing volcanic belt has been called Juroporphyria.[2] This seems to have been separate from the southern portion which may now be called Jurocalifornia, a dominantly volcanic land which lay in contact with the west margin of the main part of the Sonoran geanticline—Jurosonora. These volcanic fringes were factors in paleogeography through the Middle Jurassic and at certain times thereafter. Also, concomitantly with the building of Juroporphyria, the area to the east of it became a separate geosynclinal basin. Schuchert has called this the Pacific Geosyncline. The southern continuation of this trough lay to the west of Jurocalifornia. The Pacific geosyncline survived as such until the middle of the Cretaceous. Jurassic diastrophism seems to have had but little effect on it. However, its structural continuity was broken by the mid-Cretaceous orogeny, and utterly shattered by the Laramide orogeny. The Upper Cretaceous basins are separate and independent: the Cenozoic basins are not geosynclinal.

[37] A. Knopf: U. S. G. S., Prof. Paper 110, 1918.

[2] C. H. Crickmay: Univ. of Calif., Publ. Geol., 19, 1930.

Middle Jurassic Seas and their Faunas

General. While volcanic eruptions held sway in Juroporphyria, the sea spread notably, reaching a maximum in early Stepheoceratan time, and seemingly disappearing soon after. Its greatest expansion coincided with the cessation of vulcanism during late Sonninian and early Stepheoceratan time. Faunas of that age are rich and numerous.

Southern Alaska. In southern Alaska, occur several fine faunas, mostly undescribed. From the Kialagvik formation, largely sandstone, of Kialagvik Bay, the following have been described: "*Lillia*" *howelli*, "*Lillia*" *kialagvikensis*, "*Amaltheus*" *whiteavesii*, and some pelecypods.[36] With these, subsequent collectors have brought to light many things, including large, coarsely ribbed Pectinids, probably *Parapecten*. The generic references of the ammonites are incorrect, but are no worse than the later generic adventures of these species.[37] *Howelli* has the shell contour, ribbing, and septal line of a Polymorphitid, probably a late member of the series. Its generic affinity is not entirely certain, though plainly it is no *Hammatoceras* as is commonly suggested. *Kialagvikensis* seems to belong to *Fontannesia*. *Whiteavesii* is a late Hildoceratid, showing affinity with *Ludwigella*, but in a stage of development comparable to *Hyperlioceras*. These species suggest an early Sonninian date, though it is possible that they may not be synchronous. In any case, they are all early Middle Jurassic. So the fauna lived at a time when vulcanism was raging throughout Juroporphyria. Hence it is significant in showing that this vulcanism did not reach the region of the Alaska Peninsula.

A roughly correlative, though undescribed fauna containing large, coarsely ribbed Pectinids, etc., is found in the volcanic Talkeetna formation of Matanuska Valley.[38] Above this formation lies the Tuxedni sandstone with a rich fauna, not yet described, of late Sonninian age including "*Sonninia*" sp., "*Stepheoceras*" sp., etc.[37]

[36] C. A. White: U. S. G. S., Bull. 51, 1889.
[37] G. C. Martin: U. S. G. S., Bull. 776, 1926.
[38] G. C. Martin and F. J. Katz: U. S. G. S., Bull. 500, 1912.
[37] G. C. Martin: U. S. G. S., Bull. 776, 1926.

Far richer, though undescribed, Sonninian faunas are listed from the Tuxedni sandstone of the west shore of Cook Inlet [40] where the formation is from 1500 to several thousand feet thick.

Juroberingia. In the Alaska Range, the Tordrillo Sandstone yields a small fauna of the same age. It is noteworthy that this formation is of coarser grain in general than the Tuxedni. This fact shows that the Tordrillo was deposited nearer to the source of the sediment, which thus appears to have been central Alaska. This is vitally important, because it has already been suspected that central Alaska, being free from Jurassic deposits, was a permanent land mass during the whole period. This land will be called Juroberingia. It seems to have been a northeasterly extension of Jurangara. Further, the complete lack of Jurassic marine deposits from the borders of the Bering Sea suggests strongly that there was then no "Bering Portal." Marine connection between Pacific and Arctic was probably across the Yukon (not only through the Jurassic but also in the early Cretaceous), that is, round the east end of Juroberingia.

Southeastern Alaska. Other small early Middle Jurassic faunas are reported from southern Alaska. However, it is hardly needful to mention any but that from Windfall Harbour, Admiralty Island, where a sandstone interbedded with volcanics yields the large, coarsely ribbed species of *Pecten*.[27] This is important chiefly in indicating the conditions far to the southeast of the other Alaskan localities.

Western British Columbia. Three early Middle Jurassic faunas occur on Skidegate Inlet in the Queen Charlotte Islands: [41] The *Zemistephanus richardsoni* fauna including species of *Kanastephanus* and *Teloceras*, the *Defonticeras defontii* fauna including six species of that genus, and the *Itinsaites itinsæ* fauna. These three faunas are of late Sonninian date, but are not yet exactly correlated. The

[40] G. C. Martin and F. J. Katz: U. S. G. S., Bull. 485, 1912.
[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.
[41] F. H. McLearn: Bull. 54, Nat. Mus. Canada, 1929.

locality was probably near the western margin of Juroporphyria, for the deposits are notably thinner than in the British Columbia Coast Range.

Some Middle Jurassic localities occur in the Bulkley Valley, British Columbia, whence the following are known: *Sonninia hansoni* McLearn, *Sonninites silveria* McLearn, *Guhsani ramata* McLearn, and various pelecypods.[42] These have not been separated in the field, but nevertheless they mark faunas of different ages—Sonninian, *sauzei*, to Stepheoceratan, *Epalxites*.

Another locality occurs on Harrison Lake, where *Entolium vulcanicum* and *Belemnopsis themis* occur in beds of argillite and arkose well down in the volcanics.[43]

Still another is the isolated occurrence of "*Stepheoceras*" *nicolense* Whiteaves near Nicola.[44]

Break in the Vulcanism. These occurrences mark an important cessation of the Middle Jurassic volcanic activity, lasting from late Sonninian to early Stepheoceratan time.

Interior of British Columbia. A very important occurrence of Jurassic rocks is that at Ashcroft, where there is a considerable Middle Jurassic succession:[1]

Basque conglomerate	1130 feet
Opuntia formation, mostly dark shales, yielding a rich fauna which includes *Archæodon phylarchus* Crickmay *Gervillia ashcroftensis* Crickmay *Scaphogonia argo* Crickmay *Kallistephanus* sp.	1370 feet
Ntlakapamux formation, 200 feet sandy, crinoidal limestone yielding: *Parapecten ntlakapamuxanus* Crickmay *Fontannesia* cf. *carinata* Buckman etc., underlain by conglomerate	470 feet

The *Fontannesia* fauna is of early Middle Jurassic date: Sonninian, "*discitæ*," and is probably correlative with the

[42] F. H. McLearn: Bull. 44, G. S. C., 1926.
[43] C. H. Crickmay: Nat. Mus. Canada, Bull. 63, 1930.
[44] J. F. Whiteaves: *Ottawa Nat.*, 23, 1909.
[1] C. H. Crickmay: Univ. of Calif. Publ. Geol., 19, 1930.

Kialagvik Bay fauna of Alaska. Its occurrence at Ashcroft shows that the seas crept through the volcanic belt and inundated the interior of British Columbia while the eruptions were still in progress. There are no volcanics in the Ashcroft deposits: the locality lay too far east of Juroporphyria. The *Kallistephanus* fauna belonged to the period of cessation of vulcanism, that is, Sonninian, *sauzei*. The conglomerates seem to have been derived from a land area only a short distance to the east—the Cordilleran Intermontane Geanticline.*

Oregon. Several very remarkable, undescribed faunas occur in the Middle Jurassic of Central Oregon. They are now being studied by Mr. R. L. Lupher. It is hoped that he will soon make them known.

California. On Pit River, California, the Potem formation, about 2000 feet thick, lying on the early Middle Jurassic Bagley andesite, consists, in its lower part, of sandstone and shale, with Middle Jurassic fossils; and in its upper, of tuffs and agglomerates which mark the late Middle Jurassic vulcanism.[45]

At Mt. Jura, California, there is a notable Middle Jurassic sequence. In descending order:

Red shale and agglomerate	1000 feet
Conglomerate	200 "
Black shale	100 "
Mormon sandstone, highly fossiliferous	300 "
Stratified tuffs with corals	100 "
Brown shale	80 "
Thompson limestone (restricted)	20 "
Red shale with *Nerinea* sp.	30 "
Fant andesite flows	800 "

The faunas have been listed in part by Hyatt,[28] and some of their species have been described by Meek [29] and by W. B.

* Complete argument under Jurozephyria.

[45] J. S. Diller: U. S. G. S. Folio 138, 1906.

[28] A. Hyatt: G. S. A., Bull., Vol. 3, 1892.

[29] F. B. Meek: *Palæont. Calif.*, Vol. 1, 1864.

Clark.[46] The Mormon fauna is the only important one. It includes:

Cidaris californicus Clark
Hemicidaris intumescens Clark
Pseudodiadema emersoni Clark
Stomechinus hyatti Clark
"Rhynchonella" gnathophora Meek
"Terebratula" cf. perovalis Sowerby
Inoceramus obliquus Meek
Inoceramus rectangulus Meek
Trigonia pandicosta Meek
Astarte ventricosa Meek
Unicardium gibbosum Meek
"Sphæroceras" cf. gervilli
"Grammoceras" sp. (= "Sonninia" spp. nov.)

The "*Grammoceras*" sp. is the immature shell of large mammillate Sonniniids of which Hyatt apparently failed to find mature examples. The faunas are early Middle Jurassic as Hyatt concluded—late Sonninian to early Stepheoceratan. The Mormon and associated formations mark the late Sonninian break in eruptive activity.

Faunas of this age have not been reported from farther south in the Sierra Nevada, but they occur there none the less. Mr. S. G. Clark has shown the writer an undescribed species of *Posidonomya* and fragments of Sphæroceratids from a locality in Placer County where they are associated with volcanics.

In Summary. These occurrences are the records of a nearly continuous seaway, broken only here and there by volcanic protuberances, and covering the entire coastal belt from Alaska to California. The same sea extended northward across the Yukon to the Arctic, and eastward into the Rocky Mountain region. Records of a sea of the same date appear in southern Mexico.[47]

The Far North. On the Arctic slope of Alaska, the Kingak shale yields a Sonninian fauna probably equivalent to that of the Kialagvik formation of the Alaska Peninsula.[48]

[46] W. B. Clark and M. W. Twitchell: U. S. G. S., Monog. 54, 1915.
[47] J. Felix and H. Lenk: *Beitr. Geol. Palæont. Mexiko*, 1890–99.
[48] E. de K. Leffingwell: U. S. G. S., Prof. Paper 109, 1919.

In the Yukon Territory, at Rink Rapids on the Lewes River, there occurs "*Schlœnbachia*" *borealis Whiteaves*.[49] This species is neither a *Schlœnbachia* nor a Cretaceous form. It is an advanced Sonniniid of the early Middle Jurassic.

The *Vaugonia* fauna of Divide Lake,[2] head of Portland Canal, British Columbia, is probably early Middle Jurassic. It includes *Vaugonia veronica* and *V. mariajosephinæ.*

The Rocky Mountain Region. The Devils' Lake fauna from the eastern front of the Rocky Mountains in the latitude 51° 15′, originally described as Cretaceous, is of Sonninian, *Witchellia*, date. It includes:[49, 50] "*Terebratula*" *robusta* Whiteaves; "*Rhynchonella*" sp. n.; *Lima perobliqua* Whiteaves; *Oxytoma mcconnelli* Whiteaves; "*Trigonia dawsoni*," Whiteaves; '"*Schlœnbachia*" *borealis*, Whiteaves'; "*Schlœnbachia*" *gracilis* Whiteaves; etc. The last two species are Sonniniids. Besides these species the author has collected undescribed Stepheoceratids at this locality.

From the base of the Fernie formation of Sheep Creek, Alberta, McLearn[51] has described *Stemmatoceras albertense*, *Saxitoniceras allani*, and *S. marshalli*. Along with these occur mollusks and brachiopods. The date is Stepheoceratan, about *Epalxites*.

At Blairmore, Alberta, occurs a fauna of early Middle Jurassic aspect, the date of which is uncertain:[52] *Chlamys mcconnelli*, *Lima stantoni*, *L. whiteavesi*, *Plagiostoma blairmorensis*, etc.

The fauna found by Peale in the Lower Canyon of the Yellowstone River[53] does not belong in the early Middle Jurassic as Hyatt[28] had thought. In spite of the similarity with European Inferior Oolite, its age is early Upper Jurassic.*

Faunal Geography. So much for the West American

[49] J. F. Whiteaves: *Contrib. Canada. Pal.*, I, pt. 2, no. 4, 1889.
[2] C. H. Crickmay: *Univ. of Calif. Publ. Geol.*, Vol. 19, 1930.
[49] J. F. Whiteaves: *Contrib. Canad. Paleont.*, I, pt. 2, no. 4, 1889.
[50] F. H. McLearn: G. S. C., Summ. Rept., 1922, 1923.
[51] F. H. McLearn: G. S. C., Bull. 49, 1928.
[52] F. H. McLearn: *Trans. R. S. C.*, 3d Ser., Vol. 18, 1924.
[53] F. B. Meek: Hayden Survey, 6th Ann. Rept., 1873.
[28] A. Hyatt: G. S. A. Bull., Vol. 3, 1892.
* See page 43.

distribution of these faunas. Much remains to be known of their relationships and their derivation. Certainly some of the Sonniniids, Sphæroceratids, and Stepheoceratids are very close to, or identical with, western European species. On the other hand, most of the species and even genera of these three families are peculiarly west American. But the story of the actual intermigrations is still obscure. Not even the route of migration is known.

History. From the evidence presented, it can be seen that the early Middle Jurassic seas were quite extensive, covering, at least at one stage, the whole Pacific border from California to Alaska, and filling much of the Rocky Mountain region. But of their later history little is known. Certainly, in the Juroporphyria region they were blotted out by vulcanism which prevailed until late in the Middle Jurassic. East of the volcanic region the beds containing the Sonninian and Stepheoceratan faunas are succeeded first by a notable thickness of unfossiliferous strata, and then in turn by Proplanulitan and later fossiliferous deposits. Whether the seas withdrew or remained during the later Middle Jurassic is an unanswered question. The utter absence of faunas between Stepheoceratan and Proplanulitan is very puzzling. Did the volcanoes bar marine animals from entering the epeiric seas at this stage? Or, was the continent highly emergent? The latter seems more likely.

Jurozephyria and the Southern Rocky Mountain Basin

Jurozephyria: Its Origin. The distribution of Middle Jurassic fossil localities is such, that although they occur all along the Pacific border and in the Rocky Mountains, none are found in the area of a narrow belt coinciding with the Gold Ranges of British Columbia, and extending south through Idaho, Western Utah and Nevada, to Arizona and Sonora. This belt is indeed entirely without evidence of Jurassic deposits. This led Schuchert to regard it as a permanent land area during Jurassic time. Evidence confirming Schuchert's view is to be seen in the coarse clastic deposits which surround

the area. The coarse beds in the Jurassic of the Rocky Mountains,[11, 14, 54] and their striking contrast in this respect to the Jurassic of the Great Plains, indicates derivation of rock waste from the west; and so, by inference, a rising land to the west of the site of the present day Rocky Mountains.

The western slopes of this rising land received similar coarse sediments though they are known from very few places. The occurrence of a "breccia," at the base of the Jurassic in western Nevada, has been mentioned already.[12] Farther north, namely at Ashcroft, British Columbia, the Jurassic includes some conglomerates of considerable significance. These contain plutonic pebbles. No such material could have come from the volcanic regions to the west where plutonic pebbles are found in no conglomerates older than Cretaceous. It is therefore concluded that the pebbles came from the east. But in the Rocky Mountain region plutonic pebbles are unknown in the Jurassic. Hence, it seems likely that the pebbles came from a source between the Rocky Mountains and Ashcroft. Moreover, many of the plutonic fragments are large and ill rounded as though they had travelled but a short distance. All of which seems to indicate that not far east of Ashcroft, namely in the region of the Gold Ranges, a land area rose notably, so as to be deeply eroded, during the Jurassic.

The land to which all these evidences point is the Cordilleran Intermontane Geanticline which grew from the Sonoran Geanticline of the early Mesozoic. As a Jurassic land, the main, or southern, part of this protaxis may be called Jurosonora. The northern extension of it has been called Jurozephyria.[2] This is really Neumayr's [55] "Halbinsel Uta"; but it is so much larger and more important than Neumayr even suspected, extending as it did from Nevada to British Co-

[11] W. T. Lee: Smithson. Misc. Coll., 69, 1918.
[14] J. B. Reeside, Jr., and J. Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.
[54] F. H. McLearn: G. S. C., Summ. Rept., 1915, 1916.
[12] H. W. Turner: *Am. Geol.*, 29, 1902.
[2] C. H. Crickmay: *U. of Calif. Publ. Geol.*, 19, 1930.
[55] M. Neumayr: *Denkschr. K. K. Akad. Wiss. Wien*, 50, 1885.

lumbia, that his name seems inappropriate, and the new name justifiable.

Jurozephyria was merely a stage in the growth of this enormous protaxis. Extension in a northerly direction gave the Rocky Mountain Geosyncline its separate existence as *such*. The whole axis continued to rise through the Jurassic, the seas passing round its northern end. Finally, by the end of the period, it attained such complete spatial continuity and permanence, as to shut all Pacific invasions out of the Rocky Mountain Geosyncline.

The Southern Rocky Mountain Basin. The rising axis, Jurozephyria, was the most important geographic feature, next to the continental nucleus itself, in the Jurassic Period. Its first notable rising shut the Upper Triassic seas out of the Colorado Plateau region which thereby became an inland basin. The Upper Triassic Shinarump and Chinle formations were deposited in that basin by streams flowing northward and eastward from Jurosonora, and possibly westward from the "ancestral Rocky Mountains" of central Colorado. Some disturbance of the axis probably marked the close of the Triassic—that which is registered by the Chinle-Wingate discordance. The supposedly Lower Jurassic sediments—mainly sandstones, thickening toward the west,—suggest by their thickening and becoming coarser in that direction that they were derived largely from the west. This means a shifting in the location of the highlands from mainly south to mainly west of the basin, concomitant with the extension northward of the Sonoran Geanticline. Good reviews of evidence are found in the works of Lee [11] and others,[14] though revisions of correlations have been going on so rapidly lately that many discrepancies are naturally to be found between interpretations made only a few years apart.

The Jurassic deposits of this region (Glen Canyon Series, etc.) give every evidence of deposition in an immense flat desert basin.[14] Some of the sand and clay was deposited by

[11] W. T. Lee: Smithson. Misc. Coll., 69, 1918.

[14] J. B. Reeside and J. Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.

[14] J. B. Reeside and J. Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.

shifting streams. Some is dune sand. The upper parts of the series (Navajo sandstone) are remarkably crossbedded and contain dreikanter. The few limestones seem to be the deposits of standing bodies of fresh water like those of the Morrison formation.

The interpretation of the physiographic environment of such deposits is difficult only so long as we fail to realize the fundamental differences between the Jurassic continent and that of the present day. In the Jurassic, most of the area of the continent was without relief, and had only enough elevation to maintain its surface above sea-level—so low, indeed, as largely to inhibit subaërial erosion. Such a conclusion is inevitable; for what else but permanently low, flat surfaces could make possible the incursion, without notable erosion, of one epeiric sea after another, and the deposition of flood plain deposits over hundreds of thousands of square miles?

The Contrast with Modern Physiography. On the other hand, no epeiric sea, comparable to any of the great ones of the past, could appear today until the continent were profoundly truncated by erosion. If such erosion occurred the better part of the geologic record would disappear. Of the Jurassic and later rocks only the merest vestiges would remain. Then, if the seas spread over the land, the new deposits formed therein would lie upon the bevelled roots of our mountains, and upon the worn basements of our plains. They would lie on an universal unconformity more profound than any of Paleozoic or later time, like only that which delimits the Archæan from later rocks. Indeed, the modern continent, with its high elevation and complex heterogeneity of surface and structure, may be comparable to its Archæan ancestor. But, certainly, it is in no way similar to the low, flat continent of Jurassic time.

The Callovian Seas and their Faunas

General. The vulcanism came to an end everywhere toward the close of the Middle Jurassic, and an extensive sea

soon overspread, not only the Pacific border, but both the Pacific and Rocky Mountain geosynclines. The deposits of this sea are mostly dark shales—some are veritable "black shales." Similar faunas are known from southern Alaska to the northern United States. In California somewhat different faunas occur.

Alaska. In Alaska, the Chinitna shale on Oil Bay, Cook Inlet, is about 2300 feet thick and overlies directly the Middle Jurassic Tuxedni sandstone. The shale yields *Cadoceras doroschini* Eichwald, *C. schmidti* Pompeckj, and *C.* cf. *stenoloboide* Pompeckj.[27] Of the other genera of ammonites reported by Martin, the majority appear to be misidentifications. It is desirable to suggest that such names as *Oxynoticeras*, *Harpoceras*, *Sphæroceras*, and "*Stephanoceras*" when quoted in a list of Upper Jurassic fossils should be enclosed in quotation marks; so as to indicate that, not these genera, but forms superficially similar, are present. Otherwise, the list, like Martin's, is very misleading to the uninitiated. The "*Oxynoticeras*" is a Callovian oxynote, no relation to the Sinemurian genus. The "*Harpoceras*" may be an Oppelid. The *Sphæroceras* and *Stephanoceras* may be expected to be Macrocephalitids and Gowericeratines respectively.

The *Cadoceras* fauna occurs also in Matanuska Valley where the Chinitna shale yields *C. doroschini*, and indeterminate species.[56]

On the Alaska Peninsula, the Shelikof formation, 6000 feet thick, mostly shale, yields the following from various localities near Kialagvik and Cold bays:[57, 58, 59, 27] *Cadoceras wosnessenskii* Grewingk, *C. doroschini* Eichwald, *C. grewingki* Pompeckj, *C. schmidti* Pompeckj, *C. petelini* Pompeckj, *C. stenoloboide* Pompeckj, *Phylloceras subobtusiforme* Pompeckj,

[27] G. C. Martin: Bull. 776, U. S. G. S., 1926.
[56] Paige and Knopf: G. S. A. Bull., Vol. 18, 1907.
[57] C. Grewingk: Russ-kais. Min. Ges., Verh., 1850.
[58] E. Eichwald: Bemerkungen, Halbinsel Mangischlak, Aleutischen Inseln., 1871.
[59] J. F. Pompeckj: Russ-kais. Min. Ges., Verh., 1900.
[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.

certain gowericeratines and Macrocephalitids, and other forms misidentified by Eichwald.[58]

The writer cannot agree with Buckman's opinion that the forms referred to *Cadoceras* are really Macrocephalitids.[60] He suspects strongly that Buckman had no actual material from Alaska, and judged only from published illustrations. The species in question are to be distinguished from Macrocephalitidæ, and united with Cardioceratidæ, by their septal lines: q. v.; their development: serpenticone or platycone to cadicone or sphærocone; and by ribbing: secondaries never more than twice as numerous as primaries. In the collections seen by the writer there are several genera of these early Cardioceratids.

Martin has made a praiseworthy attempt to distinguish the localities mentioned by the early Russian writers, but for all his success, it is hardly possible to discern anything, from the literature, of the association or succession of species.

British Columbia. Similar faunas occur on Skidegate Inlet, Queen Charlotte Islands, in the sedimentary beds of sandstone and shale which lie upon the Yakoun volcanics. These have been studied lately by McLearn who describes:[31, 41, 61] *Seymourites plenus, S. multus, S. loganianus* (Whiteaves), *Yakounoceras gitinsi, Y. abruptum, Y. ingrahami, Y. torrensi, Galilæites penderi, "Torricelliceras" newcombei* (Whiteaves), McLearn.

Faunas of this age occur on Harrison Lake,[42] in the Mysterious Creek formation which consists of 2500 feet of black shale overlying the middle Jurassic volcanics. In the lower part of this formation occur Macrocephalitids of *Catacephalites* aspect. Somewhat higher, or about 1500 feet above the base, is found the *Cadoceras* fauna consisting of

[58] E. Eichwald: Bemerkungen, Halbinsel Mangischlak, Aleutischen Inseln., 1871.

[60] S. S. Buckman: Nat. Mus. Canada, Bull. 58, 1929.

[31] F. H. McLearn: *Trans. R. S. C.*, XXI, 1927.

[41] Bull. 54, Nat. Mus. Canada, 1929. There seems no purpose in reviewing previous work on the Jurassic, and its confusion with Cretaceous, on the Queen Charlotte Islands. See McLearn's works, and

[61] J. D. Mackenzie: G. S. C., Memoir 88, 1916.

[42] C. H. Crickmay: Nat. Mus. Canada, Bull. 63, 1930.

Cadoceras schmidti Pompeckj, *C. brooksi* Crickmay, *Paracadoceras harveyi* Crickmay, etc. Still higher, near the top of the formation, another Macrocephalitid fauna occurs. This includes *Lilloettia lilloetensis* Crickmay, *Lilloettia mertonyarwoodi* Crickmay, *Buckmaniceras buckmani* Crickmay, *Anomia columbiana* Crickmay, etc.

In the northern Skagit Range, British Columbia,* "*Trigonia*" *plumasensis* (Hyatt) Packard occurs associated with undescribed Macrocephalitids, in a "black" shale formation.

At Ashcroft, in the interior plateau of British Columbia, the black shales of the Minabariet formation, 1000 feet thick, yield various Gowericeratines of Proplanulitan age.[2]

Pacific States. Degenerate Macrocephalitids, probably of a late Proplanulitan date, have been collected near the town of Mineral on the western border of Idaho.†

At Mt. Jura, California, occur faunas of about the same age of a somewhat different character. These are the faunas of the Bicknell "sandstone" and Hinchman tuff.[28, 36] Studies of these faunas, now being prosecuted by the writer, seem to show that they are closer in age than Hyatt thought—probably both Callovian. From the Bicknell, Hyatt listed: "*Trigonia*" *obliqua* Hyatt, "*T.*" *plumasensis* Hyatt, "*T.*" *naviformis* Hyatt, "*Rhacophyllites*" sp., "*Macrocephalites*" sp.; from the Hinchman: a number of species of *Stylina*, *Camptonectes bellistriatus*, "*Rhacophyllites*" sp.

It is not yet possible to correlate these accurately, but the author is now working on the fossils of Mt. Jura with a view to producing a monographic revision. The faunas give every promise of yielding much to a detailed study.

"*Olcostephanus*" *lindgreni* Hyatt [18] is close to *Galilæiceras* and hence of Callovian age. This dates part of the Colfax shales of Colfax, California, as early Upper Jurassic.

In Summary. These occurrences, from Alaska to Cali-

* C. H. Crickmay: Field work, 1926, 1929.
[2] C. H. Crickmay: U. of Calif. Publ. Geol., 19, 1930.
† Mrs. R. L. Lupher: personal communication.
[28] A. Hyatt: G. S. A. Bull., Vol. 3, 1892.
[36] J. S. Diller: U. S. G. S., Bull. 353, 1908.
[18] A. Hyatt: G. S. A. Bull., Vol. 5, 1894.

fornia, outline the Callovian inundation of the coastal belt and the Pacific Geosyncline.

Rocky Mountain Region. Quite a separate series of similar occurrences are to be noted in the Rocky Mountain region. From the Fernie shale, on Cascade River near Banff, Alberta, the writer has collected Proplanulitan Macrocephalitids and Gowericeratines which have not yet been described.

From the Fernie, on Kananaskis River, McLearn[51] has described *Seymourites mcevoyi*.

The Fernie formation at Blairmore, Alberta, 1000 feet thick, mainly of dark shales, yields from its middle part a considerable fauna, including: *Paracephalites jucundus* Buckman, *P. glabrescens* Buckman, *Metacephalites metastatus* Buckman, *Miccocephalites miccus* Buckman, *M. laminatus* Buckman, *M. concinnus* Buckman, and a number of pelecypods.[52, 21, 60] These are certainly of Proplanulitan age.

The writer has collected undescribed Proplanulitan Gowericeratines from the black shales of the East Butte of the Sweet Grass Hills, Montana. This, he regarded as the southern limit of these faunas, but field work in the summer of 1930 has shown that the fauna of the Lower Canyon of the Yellowstone River is of this age.[53, 62] The common species of this fauna which occurs also in Yellowstone Park, southeastern Idaho, and southern Utah, are:[63, 14]

Gervillia montanaensis Meek
Modiolus subimbricatus Meek
Trigonia americana Meek
T. montanaensis Meek
Pleuromya subcompressa Meek
Pholadomya Kingii Meek
Goniomya montanaensis Meek
Cardinia præcisa White

[51] F. H. McLearn: Bull. 49, G. S. C., 1928.
[52] F. H. McLearn: *Trans. R. S. C.*, 18, 1924.
[21] F. H. McLearn: *Trans. R. S. C.*, 21, 1927.
[60] S. S. Buckman: Bull. 58, G. S. C., 1929.
[53] F. B. Meek: Hayden Survey, 6th Ann. Rept., 1873.
[62] C. A. White: Hayden Survey, 12th Ann. Rept., 1883.
[63] G. R. Mansfield: U. S. G. S., Prof. Paper 152, 1927.
[14] J. B. Reeside, Jr., and J. Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.

With these the writer has found *Peltoceras* sp. indet. which fixes their age roughly as early Upper Jurassic. However, further work is urgently needed on some of the localities, for instance, southern Utah whence Reeside and Gilluly report *Cardioceras* with these Callovian pelecypods—an inexplicable association.

Faunal Geography. It seems remarkable that the three dominant families of early Upper Jurassic ammonites—Cardioceratidæ, Macrocephalitidæ, and Kosmoceratidæ (Gowericeratinæ)—though contemporaries, have rarely been found together, or even in the same stratal section, in North America. As a result, very little is yet known of the exact chronologic relations of genera and species. Investigation of this problem is urgently needed. As to the origin of the faunas, these seem to have arisen in the Arctic: all the records of "*Cadoceras*," the Gowericeratines, and the late forms of catagenetic Macrocephalitids are circumboreal.

The Upper Jurassic Vulcanism

British Columbia. After the Callovian seas had attained their maximum, a third period of vulcanism supervened. One record of this occurs in the Jurassic Series on Harrison Lake. The Billhook formation is a tuff, 1800 feet thick, yielding *Haidaia* aff. *dawsoni* Whiteaves, *H. packardi* Crickmay, *H. billhookensis* Crickmay, and an undescribed Cardioceratid near to *Cadoceras*, only more advanced.[43] This means a late Callovian date.

A nearby record is the tuffaceous beds which form the top of the Dewdney formation in the Coquihalla River area.[21] The fauna of these tuffs is not yet described. Hence their age is uncertain.

The Iltasyouco River faunas [64] are of late Callovian age. They include: "*Gowericeras*" *pluto* (Whiteaves), *Haidaia dawsoni* (Whiteaves), *Pinna subcancellata* Whiteaves, and

[43] C. H. Crickmay: Nat. Mus. Canada, Bull. 63, 1930.
[21] C. E. Cairnes: G. S. C., Mem. 139, 1924.
[64] J. F. Whiteaves: Appendix, G. S. C., Rept. of Prog., 1876–77, 1878.

Grammatodon iltasyoucoensis Whiteaves, and a number of other ammonites and pelecypods probably misnamed in Whiteaves' lists. The fossils occur in tuffaceous formations, not yet exactly correlated, but in general of early upper Jurassic age.

California. In the much confused Upper Jurassic of Mt. Jura, California, there are volcanics of this age: * the Hinchman tuff, Bicknell "sandstone," and associated formations. These include both fine and coarse tuffs of Callovian age.

Rocky Mountain Region. In the Rocky Mountain region, tuffs of an uncertain date are reported in the post-Callovian Jurassic.[54, 65] The stratigraphic position of these corresponds to that of the records of vulcanism discussed above, though their exact correlation is still uncertain. They may be wind-carried ash beds, correlative with this vulcanism.

Alaska. In Alaska, there is no known record of any vulcanism of this epoch, unless the volcanics included with the Naknek formation on Cook Inlet are of this age.[27]

Summary. This early Upper Jurassic outburst of vulcanism seems to have occurred chiefly in the areas of the Middle Jurassic vulcanism. Apparently, it was more confined areally, of less power, and of shorter duration. It brought the Callovian marine expansion to an end, and prefaced an extensive, though little known, period of mountain-building which took place in early Upper Jurassic time.

The Early Upper Jurassic Orogeny

Harrison Lake. At Harrison Lake, the Agassiz Series (the sediments bearing the Argovian faunas) rests unconformably upon the earlier Upper Jurassic. The basal part of the Agassiz Series is a thick conglomerate called the Kent formation. The conclusion is unescapable—deformation and uplift between the Callovian and Argovian ages.[43]

* C. H. Crickmay: Field work, 1928 and 1929.
[54] F. H. McLearn: Summ. Rept. for 1915, G. S. C., 1916.
[65] F. H. McLearn: Bull. 58, Nat. Mus. Canada, 1929.
[27] G. C. Martin: Bull. 776, U. S. G. S., 1926.
[43] C. H. Crickmay: Nat. Mus. Canada, Bull. 63, 1930.

Other Localities. At no other place has any discordance been observed, but at many places there is a striking change at this horizon toward coarseness in sedimentation. For instance, at Ashcroft a disturbance of some sort is recorded by the presence of the coarse grained Black Canyon formation [2] in a shale series. Similarly, in the California Coast Ranges, certain coarse sediments of the Franciscan Series,[66] of local origin, are probably evidence of an Upper Jurassic uplift in that region. In the Rocky Mountain region, *Cardioceras canadense* was described from a coarse grit in a formation mainly composed of dark shale.[67] And in southern Alaska, the base of the series bearing the Argovian faunas is a thick conglomerate, known on Cook Inlet as the Chisik formation. The overlying Naknek formation is described as largely arkosic. Other records are meagre, but the evidence of a disturbance, incomplete though it be, is unmistakable.

Conclusions. The coarse sediments are very variable in thickness, but they are widespread, and their total volume is therefore great. This is important. Mere thickness of a coarse deposit is no measure of the magnitude or areal extent of the uplift from which it was derived, but only of the depth of the local subsidence which permitted it to form. The volume of a coarse deposit, taken in relation to the area of the uplift from which it was eroded, is a measure of the magnitude of the uplift. It may, therefore, be concluded that this early Upper Jurassic (pre-Argovian) uplift was of some magnitude and of wide extent, affecting, as it did, the Coast Range region of California, Juroporphyria, Jurozephyria, and Juroberingia. For convenience of reference, it is now named the Agassiz Orogeny.

It is not yet possible to discern any definite trends of mountain structure: probably no strong folding occurred. However, some folding is attested by the relations at Harrison Lake.[68] There is a possibility that some, or even all, of the

[2] C. H. Crickmay: Univ. Calif. Publ. Geol., 19, 1930.
[66] E. F. Davis: Univ. Calif. Publ. Geol., 11, 1918.
[67] J. F. Whiteaves: *Ottawa Nat.*, 17, 1903.
[68] C. H. Crickmay: Stanford Univ., Abstracts of Dissertions, I, 1927.

Upper Jurassic batholiths may be connected with this orogeny instead of with the mountain building which occurred at the end of the period. So far, no evidence on this problem has been brought to light.

The Logan Sea, and other Argovian Seaways

Rocky Mountain Region. The Agassiz orogeny seems to have had very little effect beyond its own immediate province. For, in the Rocky Mountains, we meet with several faunas intermediate between the Proplanulitan and the Cardioceratan ages, as though the seaway had persisted from the one age to the other. One of these is *Peltoceras occidentale* Whiteaves from Red Deer River, western Alberta.[69] Not much later is "*Quenstedticeras*" *collieri* Reeside [70] from the Little Rocky Mountains, Montana, for which Reeside has suggested a Divesian date. Then follow the peculiar Divesian Cardioceratids found in eastern Wyoming: "*Quenstedticeras?*" *hoveyi* Reeside, "*Q?*" *suspectum* Reeside, "*Q?*" *tumidum* Reeside, "*Q?*" *subtumidum* Whitfield & Hovey, and "*Cardioceras?*" *latum* Reeside. These seem to be pre-Cardioceratan forms. Their restricted distribution is not yet explained.

These are followed by the Argovian faunas, and a notable spreading of the seas. But even these are not all of one date, as Buckman has lately indicated.[71] The first Cardioceratan faunas are those with "*Cardioceras*" *hyatti* Reeside, "*C.*" *stantoni* Reeside, "*C.*" *wyomingense* Reeside, "*C.*" *distans* Whitfield, etc.[70] These are earlier than "*C.*" *cordiforme* Meek and Hayden, "*C.*" *whitfieldi* Reeside, "*C.*" *sundancense* Reeside, and "*C.*" *stillwelli* Reeside. And these in turn are earlier than such forms as "*C.*" *whiteavesi* Reeside and "*C.*" *haresi* Reeside which may be either of late Cardioceratan or Perisphinctean age. But the work of separating the several successive faunas has never been done.

[69] J. F. Whiteaves: *Ottawa Nat.*, 21, 1907.
[70] J. B. Reeside, Jr.: U. S. G. S., Prof. Paper 118, 1919.
[71] S. S. Buckman: Type Ammonites, V, p. 69, 1925.
[70] J. B. Reeside: U. S. G. S., Prof. Paper 118, 1919.

Associated with these ammonites, there is a large fauna of pelecypods and other things. This includes:

Grammatodon inornatus Meek & Hayden
Pinna jurassica Whitfield & Hovey
Camptonectes bellistriatus Meek
C. extenuatus Meek & Hayden
Ostrea strigilecula White
Gryphæa calceola var. nebrascensis Meek & Hayden
Trigonia conradi Meek & Hayden
T. poststriata Whitfield & Hovey
T. sturgisensis Whitfield & Hovey
Mytilus whitei Meek
Cypricardia bellefourchensis Whitfield
Astarte fragilis Meek & Hayden
A. packardi White
A. dacotensis Whitfield & Hovey
Protocardia shumardi Meek & Hayden
Dosinia jurassica Whitfield
Pachyteuthis densus Meek & Hayden

These occur in the Sundance shales. They are perhaps best known in the many localities round the Black Hills and in southeastern Wyoming. But, they have been found in the Rocky Mountains, and the Colorado Plateau.

From Vermillion Cliffs, northwestern Colorado, White reports *Cardioceras cordiforme* and *Pachyteuthis densus*, etc.[13]

The southern Utah occurrences are not clearly understood. The Jurassic sequence begins with the Glen Canyon Series of sandstones which seem to indicate desert conditions: on basis of Æolian cross-bedding, and presence of dreikanter.[14] These are overlain by the Carmel formation with marine fossils, but with a puzzling mixture of types. These include "*Cardioceras*" cf. *distans* Whitfield which could hardly mean anything but a post-Callovian date, and some of the pelecypods of the Yellowstone region which are regarded as early Upper Jurassic. This deposit is overlain by the unfossiliferous Entrada sandstone, and that in turn by the Curtis formation of conglomerate, sandstone and shale which contains Sundance

[13] C. A. White: U. S. Geol. and Geog. Surv. Terr. (Hayden Survey), Vol. 10, 1878
[14] J. B. Reeside, Jr., and J. Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.

fossils: *Pentacrinus asteriscus* Meek & Hayden, *Ostrea strigilecula* White, *Camptonectes stygius* White, etc. The succession is completed by the Summerville sandstone and the Morrison formation, the whole averaging 2500 feet in thickness.

A somewhat similar section is that of southeastern Idaho.[63] Here again there are two separate, successive, Jurassic marine fossiliferous deposits, neither of which has yet been exactly dated. The total thickness is about 6000 feet.

It is generally agreed that the boundaries of the Logan Sea lay somewhat beyond the present limits of the fossiliferous deposits, on the basis that it is hardly possible for the full original extent of a deposit to be preserved: erosional destruction of littoral and near shore deposits is almost universal. Lee [11] has argued for a considerable extension of the shorelines beyond these limits, on the basis of the probability that the unfossiliferous limestones and gypsum in the Jurassic sandstones on the margins of the Rocky Mountain basin are of marine origin. But this is not proved.

Rocky Mountains of Canada. The fossiliferous deposits are readily traced to the northwest across Canada and into Alaska. "*Cardioceras*" *canadense* Whiteaves,[67] from a coarse grit, presumably in the upper part of the type section of the Fernie formation near Fernie, British Columbia, is of late Cardioceratan date. It seems strange that faunas of this age are not commoner in the northern Cordillera.

British Columbia. In the interior plateau of British Columbia, beds of Argovian age near the head of Big Creek, a tributary of Chilcotin River, have yielded: "*Cardioceras*" *canadense*, Reeside, "*C.*" *lillooetense* Reeside, and "*C.*" *whiteavesi* Reeside.[70]

The Agassiz Prairie formation on Harrison Lake, mostly dark argillite, 5000 feet thick, has yielded *Phylloceras columbianun* Crickmay, "*Anacardioceras*" *perrini* Crickmay, etc.[43]

[63] G. R. Mansfield: Prof. Paper 152, U. S. G. S., 1927.
[11] W. T. Lee: Smithson. Misc. Coll., 69, 1918.
[67] J. F. Whiteaves: *Ottawa Nat.*, 17, 1903.
[70] J. B. Reeside, Jr.: U. S. G. S., Prof. Paper 118, 1919.
[43] C. H. Crickmay: Nat. Mus. Canada, Bull. 63, 1930.

The Big Creek and Harrison localities are the only two so far known in the Pacific geosyncline. Connection between this, the Rocky Mountain trough, and the Arctic, presumably, was across northern British Columbia and the Yukon. Argovian faunas should therefore be sought in the black shales of that region to confirm this theory of connection.

Alaska. In southern Alaska, the widespread Naknek formation, of variable character, but mainly composed of arkose, with conglomerate, sandstone, and shale, averaging several thousand feet thick, yields several Cardioceratan faunas, only partially described.[70] The south shore of Chinitna Bay, Cook Inlet, yields "*Cardioceras*" *alaskense* Reeside, "*C.*" *hyatti* Reeside, "*C.*" *lillooetense* Reeside and "*C.*" *martini* Reeside. The northeast shore of Oil Bay, Cook Inlet, yields "*C.*" *alaskense* Reeside, "*C.*" *distans* var. *depressa* Reeside, "*C.*" *martini* Reeside, and "*C.*" *spiniferum* Reeside. Some of these species are found at other Cook Inlet localities and at other places in southern Alaska. For instance, "*C.*" *martini* occurs on Boulder Creek, Matanuska Valley; and undescribed species of "*Cardioceras*," near Mt. Peulik and Pearl Creek on Shelikof Strait.

Conclusions. These occurrences are certainly evidence of the greatest of the Jurassic inundations, originally perceived by Neumayr,[56] and later elaborated by W. N. Logan,[73] in honour of whom it was called by Schuchert the Logan Sea.[4] It may be noted in passing, that Logan and some others, included all the known North American Jurassic faunas with those of this sea. But this is quite too groundless a conclusion. The earlier of the faunas may have lived in seas long since drained by Argovian time. Further, the Logan Sea was a very complex seaway. Its deposits in the Great Plains region are very thin, the whole Jurassic system being only 200 feet thick on the average. On the contrary, in western Wyoming, western Montana, and in the Rocky Mountains of

[70] J. B. Reeside, Jr.: U. S. G. S., Prof. Paper 118, 1919.
[56] M. Neumayr: *Denkschr. K. K. Akad. Wiss. Wien.*, 50, 1885.
[73] W. N. Logan: *Jour. Geol.*, 8, 1900.
[4] C. Schuchert: G. S. A. Bull., Vol. 20, 1910.

Canada, the Jurassic system is much thicker, varying from 1000 to 6000 feet. The contrast is simply that which distinguishes an ordinary epeiric from a geosynclinal sea. Still other parts of the Logan Sea occupied the Pacific Geosyncline where there are thicknesses up to 5000 feet.

Lee[11] has contended that this thickening of the deposits is correlative with the production of a larger supply of rock waste in the proximity of mountains. But the phenomenon is much more than that. Only by great subsidence can deposits grow to great thickness. No matter how great the supply of rock waste, the ultimate accumulation at any place remains under the control of base-level. The great thicknesses of the Cordilleran Jurassic can only be the result of geosynclinal subsidence.

One part of the Logan Sea is still unknown—the connection between Rocky Mountain Geosyncline, Pacific, and Arctic Oceans. No faunas of Argovian age have yet been discovered in northern British Columbia or the Yukon. Hence, the mapping of the sea across this region is mainly conjectural.

Faunal Geography. The "*Cardioceras*" faunas, so characteristic of the Logan Sea, did not penetrate into the Californian region. Whether they were restricted by climate, or by some other physical barrier, is not entirely certain. Climate seems the more likely: all the records of the Cardioceratids are circumboreal. If this be so, it is the more remarkable to find members of this family as far south as Colorado in the Logan Sea.

California. In California, the Argovian faunas are made up mostly of Perisphinctoids. These are related to types common in low latitudes. Moreover, in western North America, they are almost unknown north of California. At Mt. Jura, the Foreman formation which is mainly a grey argillite with quartzite beds yields: "*Perisphinctes*" sp., "*Binatisphinctes*" sp. "*Trigonia*" aff. *naviformis* and various

[11] W. T. Lee: Smithson. Misc. Coll. 69, 1918.

other pelecypoda.[56] The exact relationships of the ammonites are not yet known. However, specimens seen by the writer suggest a Cardioceratan date.

Some of the many species of Perisphinctes reported by Hyatt [18] and others may be of this age, e.g., "*P.*" *filiplex* Quenstedt? from Stanislaus and Tuolumne Rivers. Other species, "*P.*" *colfaxii* Gabb, and "*P.*" *virgulatiformis* Hyatt which is associated with *Oecotraustes denticulata* Hyatt, are much later—Kimmeridgian—and belong to the period of the general retreat of the Jurassic seas. The exact relationships and correlation of none of these is yet certain. A critical revision of the species is necessary. But however this may be, there is no longer any doubt about the general equivalence of the Mariposa, Colfax, and Foreman formations, and even part of the Franciscan Series of the Coast Ranges which has yielded perisphinctoids and "*Aucellæ*."

California Coast Ranges. The Jurassic of the Coast Ranges is one of the most difficult questions in a study of Jurassic history. A number of Upper Jurassic fossils have been found, and, in one of the places from which fossils are absent, the name Franciscan Series has been applied to sediments which are suspected of being Upper Jurassic.[74] Unluckily, later writers, not realizing the gravity of the offense, have correlated various incongruous elements with the typical Franciscan rocks; so that the name is now little better than a geological waste-basket. For instance, Davis [66] says that conglomerates in the Franciscan contain boulders of sandstone, chert, glaucophane and other schists, diabase, serpentine and granite; all of which, except the granite, were derived from other parts of the Franciscan Series. Obviously rocks of very different ages are being included incongruously in one series. If the glaucophane schists are part of this series, then certainly the conglomerates with glaucophane

[56] J. S. Diller: U. S. G. S., Bull. 353, 1908, and field work by the writer in 1928, 1929.

[18] A. Hyatt: G. S. A., Bull. 5, 1894.

[74] A. C. Lawson: U. S. G. S., Folio No. 193, 1914.

[66] E. F. Davis: Univ. of Calif. Publ. Geol., 11, 1918.

schist pebbles are not. This example should show how necessary is further analysis of the field evidence. So far, conclusions have been drawn largely from unsorted masses of evidence which, by their mere bulk, are supposed to lend weight to the argument. The result is, that in spite of all the work done, little more has been accomplished toward a *stratigraphic* understanding of these deposits than a perpetuation of the confusion regarding them.

The main facts are these. The name Franciscan belongs to the *series* of altered sandstones and associated sediments of San Francisco and Marin Peninsula. These have yielded no fossils, hence their ages is unknown. Nevertheless, they have been widely regarded, from indirect evidence, as of Upper Jurassic age. This evidence may be summarized. Near the type locality, rocks supposedly correlative with the Franciscan are overlain unconformably by the Knoxville formation the lower part of which has seemed to be of lowest Cretaceous or even Aquilonian age.* Also, fossils of Upper Jurassic? age have been found in supposed equivalents of the Franciscan near the type locality, namely, on Alcatraz Island, Angel or Goat Island, and near San Mateo. Still farther away, Davis' collections [75] from Slate's Springs, Monterey County, from the San Luis formation are certainly Upper Jurassic; and the San Luis is thought to be equivalent in general with the Franciscan. Consequently, although the dating of the Franciscan is very uncertain, the presence of Upper Jurassic deposits in the Coast Ranges is proved. Undoubtedly, older formations have been confused with the Franciscan. But on the other hand, it is impossible that the Franciscan or any part of it belongs to a conjectural period between Jurassic and Cretaceous, as Lawson suggested. The chronology of this part of geologic time is now too well known for such a possibility.

The conglomerates of the Franciscan are especially im-

* This is another obscure problem. It is discussed in the section on the "final emergence."

[75] C. H. Davis: *Jour. Geol.*, 21, 1913.

portant because they contain pebbles of local origin, that is, derived from older Coast Range rocks instead of having come from the east. This points to a pre-Cretaceous local uplift. Perhaps there was a rising axis here during the Agassiz orogeny.*

An interesting feature of the conglomerates is the presence of granitic pebbles. These undoubtedly came from granites exposed before the last Jurassic formations were formed. This raises the unanswered question of the age of the granites. There is much to commend the suggestion that they belong to the Agassiz orogeny. However, they might be younger, that is, Portlandian; or, considerably older. They are not necessarily correlative with the plutonics of the Sierra Nevada, the age of which, though post-Kimmeridgian is not accurately known.

The Final Emergence

The Draining of the Logan Sea. In the interior of the continent, the late Argovian (late Cardioceratan or Perisphinctean) faunas are the last records of Jurassic marine life. Therefore, presumably, the seas were drained from this region toward the end of the Argovian epoch. Lee's [11] conception of the conditions surrounding the retreat of the sea recommends itself as a true one. With his guidance we may picture the gradual emergence of a great flat plain of marine sediment, broken here and there by the tangled courses of lagoons and esteros, and presenting to the sea itself an irregular margin flanked by off-shore sand-bars.

British Columbia. But complete emergence of the continent was not consummated until much later, for, west of Jurozephyria, later Jurassic faunas occur in the upper parts of the formations which bear the Argovian species. In British Columbia, supposedly Jurassic *Aucellæ* are found near the headwaters of Bridge River.[76] Also *Aucella bronni* is reported from Big Creek.[21] And a supposedly late Upper

* See section of this memoir which deals with the early Upper Jurassic orogeny.

[11] W. T. Lee: Smithson. Misc. Coll. 69, 1918.

[76] W. S. McCann: G. S. C., Mem. 130, 1922.

[21] F. H. McLearn: Trans. R. S. C., XXI, 1927.

Jurassic fauna of uncertain relationships is reported from near Hazelton.[77] The fauna includes: *Aucella?* sp., and "*Pecten*" sp. (similar to the species in the *Aucella*-beds of southwestern Alaska). O'Neill does not correlate the occurrence with the local stratigraphy.

Alaska. In Alaska, the Naknek formation on Oil Bay, yields *Amœboceras* aff. *alternans*[32] the age of which is early Kimmeridgian—much later than the Cardioceratoid forms in this formation. Also, the upper part of the Naknek at this locality yields *Aucella* sp. These late Jurassic *Aucellæ* are very common in southern Alaska. Various localities along Shelikof Strait[78] yield *Aucella bronni* Rouillier, *A. pallasi* Keyserling, *A. erringtoni* Gabb, etc. *Aucella* cf. *bronni*[79] has been found even in the Nutzotin Mountains.

Oregon. In southwestern Oregon, *Aucella erringtoni* Gabb is found at many localities and various horizons throughout the two sedimentary formations of a series constituted as follows:

Galice shales with "*Perisphinctes*" sp. and *Aucella erringtoni.*

Volcanics.

Dothan sandstone with *Aucella erringtoni.*[80]

Diller tells us the section is a doubtful one: the succession may not be as it appears.

California. In California, *Amœboceras dubium* Hyatt is found in the Mariposa slate at Texas Ranch, Calaveras County.[18] *Aucella erringtoni*[81] and varieties are known from the Mariposa estate, Tuolumne, and Stanislaus Rivers, and other places in the Sierra Nevada. "*Perisphinctes*" *muhlbachi* Hyatt from Greenwood, Eldorado County, California, seems to belong to *Dichotomoceras*, and hence marks a Priono-

[77] J. J. O'Neill: Mem. 110, G. S. C., 1919.

[78] W. R. Smith and A. A. Baker: U. S. G. S., Bull. 755, 1925. Also collections seen by the writer in the University of California, Berkeley. This and other species should in the future be referred to the genus *Buchia* which supersedes *Aucella.*

[79] E. H. Moffit and A. Knopf: U. S. G. S., Bull. 417, 1910.

[80] J. S. Diller: *Am. Jour. Sci.*, 23, 1907.

[18] A. Hyatt: G. S. A. Bull., Vol. 5, 1894.

[81] F. B. Meek: *Geol. Calif.*, Vol. I, Appendix. This and other species should in the future be referred to the genus *Buchia* which supersedes *Aucella.*

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doceratan date. These are all of Kimmeridgian age. "*Perisphinctes*" *virgulatiformis* Hyatt [18] is a *Virgatosphinctoides* and hence suggests at the earliest a late Kimmeridgian date. And J. P. Smith's mention of *Simbirskites* [19] from Nashville, Amador County, suggests a Virgatitid, and therefore a late Kimmeridgian date. This can hardly be a true *Simbirskites*, or even a relative of that genus.

These have always been supposed to be the latest Jurassic faunas in the Sierra Nevada, but the author has recently had the privilege of examining the unfigured types of "*Aucella erringtoni* var. *arcuata*" Hyatt in the National Museum. These are undoubtedly of Tithonian age. In the Coast Ranges, there are also younger pre-Cretaceous faunas. The "Aquilonian" and Tithonian *Aucellæ* ascribed variously to Franciscan and Knoxville formations have no connection with the Cretaceous but occur below the sub-Cretaceous unconformity. Collections sent to the author recently from Huasna River, California, include *Berriasella* cf. *calisto* d'Orbigny, *Substeueroceras* sp., *Buchia terebratuloides* Lahusen, etc. These certainly belong to the Berriasellidan age, that is, late Tithonian. (See page 61.)

Conclusions. The existence of these various small faunas from early to late Kimmeridgian, and sporadically even to Tithonian, along the Pacific border, shows that the seas lingered here long after the Rocky Mountain geosyncline was drained. And, from the continuous migrations from the Arctic, the "*Aucellæ*" etc., we may conclude an open marine connection with that region. The fact of the later faunas being found nearer the continental margin seems to show a slow, gradual emergence. On the other hand, there is no proof that complete emergence was not consummated at the end of the Kimmeridgian. If such emergence occurred, the Tithonian deposits were formed by an independent advance of the sea in latest Jurassic time.

Mexico. On the southern slope of the continent, the

[18] A. Hyatt: G. S. A. Bull., Vol. 5, 1894.
[19] J. P. Smith: Calif. State Min. Bureau, Bull. 72, 1916.

emergence was equally late. The Kimmeridgian faunas of Malone, Texas, mark the culmination of a marine invasion from the south. These were first described by Cragin,[82] who thought them to be of Tithonian age; but Burckhardt,[83] and more recently Kitchin,[84] has shown that the fauna consists of incongruous elements—some Kimmeridgian, some post-Jurassic (Neocomian). In Central Mexico there is a much longer record,[85, 86, 87] even including Portlandian (?) and Tithonian deposits. These show a very late persistence of the seas in the central Mexican trough.

The problem of the boundaries of these Mexican seas has been studied by Böse [88] who has shown that the Upper Jurassic marine waters extended northeastward over much of Coahuila and Nuevo Leon. Also that this part of the sea was not only close to land, but in the Portlandian, close to elevated land, for the deposits (especially those of the Portlandian) include coarse sediments, even conglomerates. This elevated area lay in southern Texas and north-eastern Mexico —Llanoria.

The Jurasside or Nevadian Orogeny

History of the Problem. It was Whitney [89] who first called attention to the antiquity of the more complex structures of the Sierra Nevada, and to the bearing of this upon the existence of a Mesozoic range of mountains in that region. The upheaval which formed these mountains was formally named by Lawson,[90] the Cordilleran Revolution. Lawson attempted to show the profound character and wide areal extent of this "revolution" which he believed to have affected the entire Cordilleran region. He argued for the importance

[82] F. W. Cragin, U. S. G. S., Bull. 266, 1905.
[83] C. Burckhardt: *Centralblatt f. Min.*, 1910.
[84] F. L. Kitchin: *Geol. Mag.*, Vol. 63, 1926.
[85] C. Burckhardt: *Inst. Geol. Mex.*, Vol. 23, 1906.
[86] C. Burckhardt: *Inst. Geol. Mex.*, Vol. 29, 1912.
[87] C. Burckhardt: *Inst. Geol. Mex.*, Vol. 33, 1919.
[88] E. Böse: *Am. Jour. Sci.*, 6, 1923.
[89] J. D. Whitney: Mem. M. C. Z., 6, 1879.
[90] A. C. Lawson: *Jour. Geol.*, 1, 1893.

in this orogeny of large scale magmatic intrusion which he supposed to have resulted in the formation of the Sierra Nevada and British Columbia Coast Range batholiths. Finally, J. Perrin Smith [91] showed that the date of the orogeny was late Jurassic.

This result was confirmed by Diller and Stanton [92] who called attention to the great progressive unconformity at the base of the Cretaceous. On the west side of Sacramento Valley, the basement complex of disordered Carboniferous and older Mesozoic is overlain by the mid-Cretaceous Horsetown formation. And, in the northeastern part of the valley, a similar basement is overlain by the Upper Cretaceous Chico formation. The youngest rocks of the basement complex are Upper Jurassic; and although these are not found in contact with the *early* Cretaceous, this relation seemed to show that the deformation of the basement rocks took place in the late Jurassic. There being no positive evidence to the contrary, the idea arose that all the deformation, even in cases of profound discordances, occurred at one time. From this followed the corollary notion of the immensity of the one upheaval.

But not all these contacts are parts of one unconformity as Diller and Stanton supposed. And not only are they of different ages, but there is a possibility that each one may be the result of more than one orogeny. Hence, none tell anything distinctly about the Jurasside orogeny. For instance, the discordance between the Chico, and the complex of granite and metamorphics on the northwestern limits of the Sierra Nevada, may be the cumulative result of the disturbances in early Upper Jurassic, late Jurassic, and mid-Cretaceous time.

The contacts, so far described in the Coast Ranges, are better, but still unsatisfactory.[74, 93] There, the basal Cre-

[91] J. P. Smith: Bull. G. S. A., 5, 1894.
[92] J. S. Diller and T. W. Stanton: Bull. G. S. A., 5, 1894.
[74] A. C. Lawson: Folio 193, U. S. G. S., 1914.
[93] H. W. Fairbanks: Folio 101, U. S. G. S., 1904.

taceous rests upon disordered older rocks; and thus limits some of the deformation to pre-Cretaceous time.

However, in the Riddle district, and on the Elk River, Oregon, there are contacts between Upper Jurassic and early Cretaceous. That at Riddle, though not actually seen, is of such a nature as to show beyond all reasonable doubt that the early Cretaceous Myrtle formation overlaps the late Upper Jurassic Dothan formation with marked unconformity. Both formations are fossiliferous: there is no great doubt about their age. On Elk River, near Blackberry Creek, the Myrtle lies unconformably on a greenstone which intrudes the Dothan.[89, 94, 95] These are the best evidences in the Pacific States, so far made known, of the disturbance at the end of the Jurassic. The unconformities are sufficiently marked to show beyond doubt that tectonic mountains were formed.

Conclusions: the Extent of the Disturbance. With this as a starting point, a tentative interpretation may be made of the universal unconformity at the base of the Cretaceous rocks. The great extent of this structure makes it seem likely that mountain structures were formed over all the country between the ocean and the site of the present Sierra Nevada, Klamath, and Cascade Mountains. The folding may have been initiated by the Agassiz orogeny or even earlier, and augmented by the Jurasside. Some was certainly compressed still closer by the mid-Cretaceous (pre-Chico) compression. There seems, however, little reason to believe with some of the older geologists that any of our existing mountain ranges, such as the Sierra Nevada, were individualized in late Jurassic time. Indeed, there is abundant evidence that most of the present day mountain groups were not separated as such until very recent times.

Problem of Date. A suggestion for a revision of the dating of the orogeny was made indirectly by Pavlov [96] in his account of the Californian *Aucellæ*, in which several of the supposed

[89] J. S. Diller: *Am. Jour. Sci.*, 23, 1907.
[94] J. S. Diller and G. F. Kay: Folio 218, U. S. G. S., 1924.
[95] F. H. Knowlton: *Am. Jour. Sci.*, 30, 1910.
[96] A. P. Pavlov: Nouv. Mem. Soc. Imp. Nat. Moscov., 17, 1907.

Knoxville species were placed as Portlandian and "Aquilonian." In this, he was followed by J. P. Smith [97] who argued that the lower Knoxville should be regarded as Portlandian. Hence, we are left to infer that the pre-Knoxville unconformity represents diastrophism of pre-Portlandian, instead of latest Jurassic, date.

Knowlton [98] arrived at a similar view from a study of the Jurassic flora obtained by Diller from the Knoxville beds of northern California and southwestern Oregon. This flora included: *Dicksonia oregonensis* Fontaine, *Polypodium oregonense* Fontaine, *Tæniopteris major* Lindley and Hutton, *Tæniopteris vittata* Brongniart, *Sagenopteris goppertiana* Zigno, S. *paucifolia* (Phillips) Ward, *Nilsonia orientalis* Heer, *N. parvula* (Heer) Fontaine, *Pterophyllum nathorsti* Schenk, *P. contiguum* Schenk, *P. æquale* (Brongniart) Nathorst, *Ctenis sulcicaulis* (Phillips) Ward, *Ginkgo digitata* (Brongniart) Heer, *Taxites zamioides* (Leckenby) Seward, *Brachyphyllum mammillaria* Brongniart. These, though associated in the same beds with *Aucella crassicollis* etc. seemed to Knowlton to show the Knoxville to be of Jurassic age. Diller opposed him, however, on the grounds that the Knoxville (= Myrtle) invertebrates were probably Cretaceous; that the deposit represents sedimentation continuous into Middle and Upper Cretaceous beds; and that a master unconformity separates it from all undoubted Jurassic rocks.

Criticism of the Work of Diller and Knowlton. In speaking of the "Jurassic flora" Knowlton was confusing several floras. Analysis of his lists shows that the Oroville flora which came from beds of undoubted Upper Jurassic age in California is very distinct: only 42 per cent of its species are common to the Knoxville floras, whereas, the latter usually have from 70 to 95 per cent in common with each other. It seems strange that neither Knowlton nor Diller perceived this. It is interesting to add that only a few years later, J. P. Smith who never entered into the controversy gave a clear statement of this.[19]

[97] J. P. Smith: *Science*, N. S., 30, 1909.
[98] F. H. Knowlton: *Am. Jour. Sci.*, 30, 1910.
[19] J. P. Smith: Calif. State Min. Bureau, Bull. 72, 1916.

The real significance of the most important evidence failed to receive due emphasis from either of the controversialists. The Jurassic flora on which Knowlton so implicitly relied was found by Diller [80] and others in association with faunas from Kimmeridgian to Lower Cretaceous. Such a long ranging flora is, of course, of no value for correlation. On the other hand, the invertebrates of the Knoxville have a restricted vertical range. *Aucella piochii* s. s. and *A. crassicollis* indicate close correlations with zones in the lowest Cretaceous.

Final Solution of the Problem of Dating. What then of Pavlov's Jurassic Knoxville *Aucellæ*? The fact of the matter is that these come not from the Cretaceous Knoxville sandstone but from the pre-Cretaceous "Knoxville" shale. The relations between these two formations seem nowhere to have been evident. However, they are well shown near the Huasna River, California.* At this place, the sandstone bearing *Aucella piochii* rests with gentle discordance on shales bearing *Aucellæ* and ammonites of various Tithonian types, including certain Berriasellididæ. The unconformity is thus dated as latest Jurassic, and the mountain-making of which it is evidence coincides with the accepted limit of the Jurassic Period.

But the relation of the various Tithonian *Aucella* beds (including certain "Knoxville" shales) to the earlier formations of the upper Jurassic is not yet known. Nor is the relation of either to the problematical Franciscan Series known. Here is the need for further field study. The pertinent question is: Was there more or less continuous submergence from Kimmeridgian to Tithonian time, or was there interruption of deposition by diastrophic disturbance? The lack of coarse sediment from the deposits of Tithonian age in California favours the former suggestion, and suggests that the only orogeny was that which terminated the Jurassic Period.

[80] J. S. Diller: *Am. Jour. Sci.*, Vol. 23, 1907.

* Recently brought to light by the work of Prof. N. L. Taliaferro who kindly entrusted the writer with the study of the fossils of that locality.

Limited Areal and Structural Importance. While some authorities continued to regard the orogeny as of profound character and broad extent, it gradually became evident that the Jurasside deformation was much gentler in character, and more limited areally, than was formerly believed. Blackwelder [98] seems to have been the first to discern the fallacy in the older opinions. He went so far as to propose a new name, Nevadian Orogeny.* Special evidence gathered subsequently, especially in the northern Cordillera, tends on the whole to confirm this view.

Special Evidence from Alaska. Some very important contacts between Jurassic and Cretaceous have been found in Alaska. At Herendeen Bay,[99] Alaska Peninsula, the basal Cretaceous Aucella-bearing rocks lie conformably on the late Jurassic sediments.[100, 27] On the headwaters of Nelchina River, in Matanuska Valley, the basal Cretaceous bearing *Aucella crassicollis* lies conformably on the Upper Jurassic Naknek formation. In the Nutzotin Mountains,[101] on Chisana and White Rivers, the Lower Cretaceous *Aucella*-beds lie conformably on the Upper Jurassic *Aucella*-beds. However, in Chitina Valley,[27] the basal Cretaceous lies with gentle discordance upon the Trias. In the Central Plateau [102] and Yukon Valley, the basal Cretaceous rests with slight discordance upon Triassic and late Paleozoic rocks. In southeastern Alaska,[27] the early Cretaceous commonly lies with slight unconformity on the Trias or Jura. In general, none of the Alaskan occurrences show a profound unconformity. Many show no evidence of late Jurassic deformation.

Special Evidence from British Columbia. In the Cascade Range of southern British Columbia, especially at Harrison

[98] E. Blackwelder: *Jour. Geol.*, 22, 1914.

* The objection to "Cordilleran revolution" as being too comprehensive a term can be met as well by using "Jurasside Orogeny."

[99] W. W. Atwood: U. S. G. S., Bull. 467, 1911.

[100] W. C. Mendenhall: U. S. G. S., 20th Ann. Rept., pt. 7, 1900.

[27] G. C. Martin: U. S. G. S., Bull. 746, 1926.

[101] S. R. Capps: U. S. G. S., Bull. 630, 1916.

[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.

[102] A. H. Brooks and E. M. Kindle: G. S. A. Bull., Vol. 19, 1908.

[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.

Lake,* the early Cretaceous *Aucella* beds lie with very gentle discordance upon the various Jurassic formations, including the latest Jurassic of that region which bears *Anacardioceras perrini* Crickmay. At this locality, the basal Cretaceous is a conglomerate composed largely of quartz-monzonite pebbles which indicate that their source was to the northwest by an increase of the average size in that direction. This shows that the Upper Jurassic batholiths had by the earliest Cretaceous become unroofed by erosion. These batholiths, near Harrison Lake, intrude the Middle Jurassic rocks, but unluckily do not show their relation to the Upper Jurassic. Hence, their date is uncertain: it is not known whether they are to be correlated with the Nevadian or the Agassiz orogeny, or with neither.

The Upper Jurassic Batholiths. This brings up the general question of the dating of the Upper Jurassic batholiths. Unfortunately, very little is known about this. At no place have the batholiths been found intrusive into post-Callovian sediments. Hence, no dating more particular than Upper Jurassic is possible. There may have been more than one period of intrusion. Possibly each of the Upper Jurassic orogenies was accompanied by intrusions. The occurrence of granite pebbles in the Franciscan rocks can mean only that some granites of the California Coast Ranges were not only formed, but also denuded, before the late Upper Jurassic sediments were laid down.

Certainly much of the Cordilleran batholithic rock has no connection with the Jurassic. It belongs to later periods. Further, most of the published maps depict the areas of plutonics of various ages in the Cordillera as too large.[17, 108]

Final Conclusions. It must finally be concluded that the Jurasside diastrophism was somewhat overestimated by the older geologists. It was hardly a revolution, but rather a disturbance. Its importance has been exaggerated, partly, no

* C. H. Crickmay: Field work, 1922, 1924, 1926.

17 Bailey Willis: U. S. G. S., Prof. Paper 71, 1911.

108 S. J. Schofield and G. Hanson: G. S. C., Mem. 132, 1922.

doubt, because the effects of the Agassiz Orogeny have been confused with it. But this was an independent, and much earlier, mountain-making, succeeded by the submergence of Argovian time. Following this, general emergence marked the later part of the Jurassic. This culminated in orogeny at the end of the period. Tectonic mountains were formed along the coastal regions of British Columbia, Oregon, and California, and probably continuously east to the Sierra Nevada. It is unlikely that any of these mountains were very high or that the rock formations were very much deformed. Structural trends are to be discerned in northern California and southwestern British Columbia. They correspond to the average trends of the Cordillera. For instance, in Tzuhalem Mountain, Vancouver Island, where the Upper Cretaceous is relatively undisturbed, some of the Jurasside folds (probably augmented by the Mid Cretaceous compression) run north-northwest.[104]

Jurozephyria and Juroberingia were uplifted. It is not known to what extent they were deformed, though the unconformable overlaps of the Cretaceous upon their edges suggest at least some folding. In general, it seems unlikely that any great continuous "Cordillera" was formed at the end of the Jurassic Period, or that any of the mountains then formed had any great similarity in arrangement or distribution with those of the present day.

North America at the Beginning of the Cretaceous

General. The lasting effects of Jurassic diastrophism upon the continental structure can be discerned to some extent by contrasting the geography of the Jurassic with that of the early Cretaceous. The latest Jurassic and early Cretaceous are represented in the central part of the continent only by freshwater deposits—the Morrison,[105] Kootanie,[106] and Mattagami [107, 108] formations and their equivalents. On the At-

[104] C. H. Clapp: G. S. C., Mem. 96, 1917.
[105] C. C. Mook: *N. Y. Acad. Sci.*, 27, 1916.
[106] J. W. Dawson: *Roy. Soc. Canada, Proc. and Trans.*, 3, 1886.
[107] J. Keele: Ont. Dept. Mines, 29, 1920.
[108] F. H. McLearn: Sum. Rept., 1926, G. S. C., 1927.

lantic seaboard there are no marine deposits of this age. The Potomac Series of Lower Cretaceous freshwater deposits lies upon the eroded edges of the older formations. This is an intimation of the subsidence of eastern North America which, in the Upper Cretaceous, brought the east coast inward, across the sunken Appalachia, to the general region of the present continental margin.

The Cretaceous Marine Invasions. In the south, an invasion from the Gulf spread northward, bringing at first, only south Pacific faunas.[84] This invasion followed the Jurassic basin. In the far north, the Rocky Mountain geosyncline was not flooded; but a broad sea encroaching on all the borders of what was left of Juroberingia, crossed the Yukon, and joined the Arctic with the Pacific. This allowed the great "*Aucella* fauna" to reach western North America. From the Pacific, another wide inundation occurred. In California, this sea was confined between the region of the northern Coast Ranges, at that time a land-mass which may be called Mendocinia,[92, 109, 110] and the Sierra Nevada together with southern California which may be called Mariposia.[111] Connection with the ocean was through Oregon,[80] and across the middle Coast Ranges.[93] From Oregon, north, the flood was broader. Its eastern limits reached western Idaho * and the eastern parts of the Interior Plateau of British Columbia.[112] The batholithic mountains which had arisen in Juroporphyria remained above water and supplied coarse detritus to the surrounding seas.

Jurozephyria. Jurozephyria had been raised to form a perpetual, impenetrable barrier: no later sea has ever crossed it. The Cretaceous faunas to the west of it are different from

[84] F. L. Kitchin: *Geol. Mag.*, 63, 1926.
[92] J. S. Diller and T. W. Stanton: G. S. A. Bull., Vol. 5, 1894.
[109] G. F. Becker: U. S. G. S., Bull. 19, 1885.
[110] J. C. Branner, et al.: U. S. G. S., Folio 163, 1909.
[111] H. W. Fairbanks: *Am. Jour. Sci.*, Vol. 45, 1893.
[80] J. S. Diller: *Am. Jour. Sci.*, Vol. 23, 1907.
[93] H. W. Fairbanks: U. S. G. S., Folio 101, 1904.
* Aucellæ in possession of Mrs. R. L. Lupher, seen by the writer.
[112] A. Bowman: G. S. C., Ann. Rept., Vol. 3, 1888.

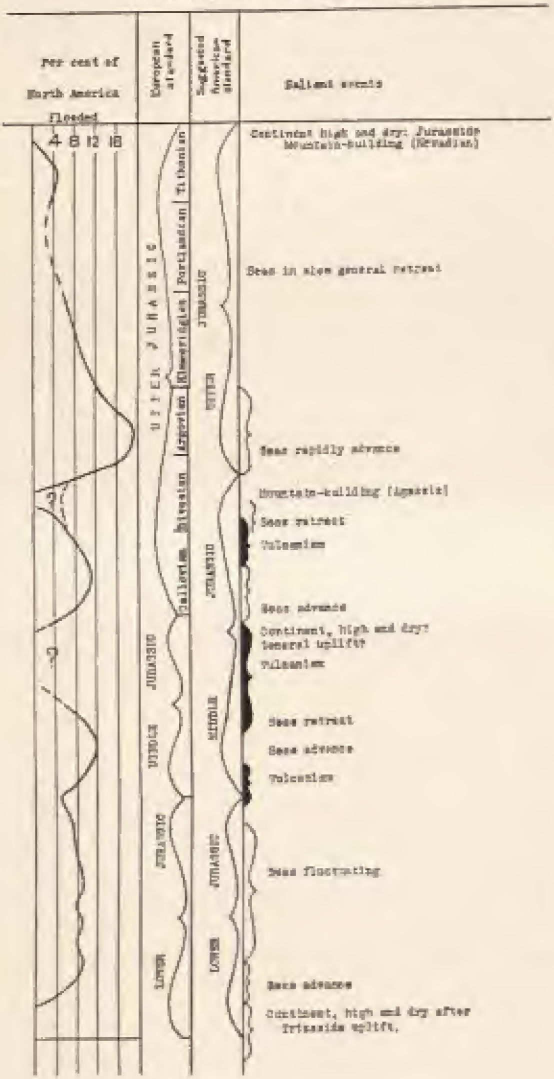
those to the east. Its height in places was marked, for coarse sediments were derived from it.[54, 14, 63, 20]

The Areas of Elevation. In summary, it may be said that six areas showed notable elevation at the beginning of the Cretaceous: the central nucleus of the continent, the Cordilleran Intermontane Geanticline which now extended from Mexico to the Arctic, the Coast Range of British Columbia which was the surviving remains of Juroporphyria, the lower plain of the Yukon River which was the much reduced vestige of Juroberingia, the northern Coast Ranges of California—Mendocinia, and the Sierra Nevada region together with southern California—Mariposia. However, much of the elevation of these areas was gone by the time the first Cretaceous deposits were formed.

Later History. The Pacific geosyncline survived until destroyed by the Mid-Cretaceous crustal disturbance, the Oregonian orogeny. The Rocky Mountain geosyncline lasted until the end of the Cretaceous. At that time the entire Cordilleran region yielded profoundly to orogenic pressure in the Laramide Revolution. There were, however, many local variations in the degree and manner of deformation; and some of these are correlative with peculiarities of composition or structure inaugurated during the Jurassic. The most profound deformation has been located in the zones where thick shales were deposited during the Jurassic. The areas of Jurassic volcanics have yielded to stresses mainly by faulting: probably they were too stiff to fold. The same is eminently true of the great bodies of Jurassic plutonics, though the latter largely resisted deformation by virtue of their own relative rigidity; and, in consequence, braced the areas they occupied, and protected formations suitably associated with them. Probably, to this is due the remarkable fact that the enormous stresses of the Laramide Revolution had practically no effect upon the Sierra Nevada or the Coast Range of British Columbia.

[54] F. H. McLearn: G. S. C., Summ. Rept. for 1915, 1916.
[14] J. B. Reeside, Jr., and J. Gilluly: U. S. G. S., Prof. Paper 150-D, 1928.
[63] G. R. Mansfield: U. S. G. S., Prof. Paper 152, 1927.
[20] R. A. Daly: G. S. C., Mem. 38, 1912.

TABULAR SUMMARY OF JURASSIC HISTORY



TABULAR SUMMARY OF JURASSIC FAUNAS

Name	Deposit	Principal Locality	Date
Berriasella cf. callisto Buchia stantoni	"Knoxville" shale	Knoxville? Napa Co., California	Berriasellidan
Virgatosphinctoides virgulatiformis	Mariposa formation	Stanislaus R., California	Pseudovirgatitan
" colfaxii	" "	Colfax, California	"
Buchia erringtoni	" "	Mariposa Estate, California	Rasenian
Amoeboceras dubium	" "	Texas Ranch, Calaveras Co., California	"
Dichotomoceras muhlbachi	" "	Greenwood, Eldorado Co., California	Prionodoceratan
"Cardioceras" spiniferum	Naknek formation	Oil Bay, Cook Inlet, Alaska	Prionodoceratan
"Binatisphinctes" sp. n.	Foreman "	Mt. Jura, California	Cardioceratan
"C." whiteavesi	Sundance "	Sundance, Wyoming	Cardioceratan, *Vertebriceras*
"Anacardioceras" perrini	Agassiz Prairie formation	Harrison Lake, B. C.	" "
"Cardioceras" canadense	Fernie formation	Fernie, B. C.	" *Goliathiceras*
"C." whitfieldi	Sundance "	Bear Lodge Butte, South Dakota	" excavatum
"C." americanum	" "	Sundance, Wyoming	" *Sagitticeras*
"C." hyatti	" "	Carbon Co., "	" cardia
"Quenstedioceras" hoveyi	" "	Bear Lodge M.s., Wyoming	Vertumniceratan?
"Q." collieri	Ellis "	Little Rocky Mts., Montana	"
Haidaia dawsoni	Porphyrite series	Iltasyouco R., B. C.	Kosmoceratan?
"Trigonia" plumasensis	Bicknell formation	Mt. Jura, California	"
"Peltoceras" occidentale	Fernie "	Red Deer R., Alberta	Kosmoceratan?
"Olcostephanus" lindgreni	Mariposa "	Colfax, California	Proplanulitan
"Gowericeras" sp.	Minnbariet "	Ashcroft, B. C.	Proplanulitan
Seymourites loganianus	Yakoun "	Skidegate Inlet, B. C.	"
Metacephalites metastatus	Fernie "	Blairmore, Alberta	"
"Trigonia" montanaensis	Ellis "	Lower Canyon, Yellowstone R., Montana	"
Lilloettia lilloetense	Mysterious Creek formation	Harrison Lake, B. C.	"
Cadoceras schmidti	Chinitna formation	Cook Inlet, Alaska	" *majesticus*
"Catacephalites" sp.	" "	" " "	" "
Stemmatoceras albertense	Fernie formation	Sheep Creek, Alberta	Stepheoceratan, *Epalxites*
Gulsania bella	Hazelton series	Hudson Bay Mtn., B. C.	" "
"Sonninia" sp.	Mormon formation	Mt. Jura, California	" "
Sonninites silveria	Hazelton series	Hudson Bay Mtn., B. C.	Sonninian, *alsatica*
Sonninia hansoni	Hazelton series	Hudson Bay Mtn., B. C.	Sonninian, *Sausei*
Kallistephanus sp.	Opuntia formation	Ashcroft, B. C.	" "
Zemistephanus richardsonii	Yakoun "	Skidegate Inlet, B. C.	"
Defonticeras defontii	" "	" " "	"
Itinsaites itinsæ	" "		"
"Schlœnbachia" borealis		Rink Rapids, Lewes R., Yukon	" *Witchellia*
Chlamys mcconnelli	Fernie formation	Blairmore, Alberta	"
Pleuromya rhynchophora	Opuntia formation	Ashcroft, B. C.	"
Fontannesia cf. carinata	Nikkapamux formation	" "	" "discitæ"
Parapecten acutiplicatus	Hardgrave "	Mt. Jura, California	Ludwigian
Fanninoceras fannini Chlamys carlottensis	Maude "	Skidegate Inlet, B. C.	Grammoceratan
"Dactylioceras" sp.	" "		Hildoceratan
"Seguenziceras" sp.	" "	" " "	Amaltheian
"Gleviceras" sp.	Harbledown "	Parson Bay "	Oxynoticeratan
Melanippites harbledownensis	" "	" " "	Asteroceratan
Arniotites vancouverensis		Crescent Inlet "	Microderoceratan
Arnioceras? nevadanum		Volcano, Nevada	Agassiceratan
Metophioceras? crossmani		" "	Coroniceratan, *rotator?*
Vermiceras? sp.	Harbledown "	Parson Bay, B. C.	Vermiceratan
"Schlotheimia" sp.		Volcano, Nevada	Schlotheimian

General Conclusions

Correlation of Jurassic Submergences. This study has shown that there were four main marine transgressions during the Jurassic: the Liassic, long lasting but not extensive; the Bajocian and Callovian, both brief but broad; and lastly the Argovian, with a marked early maximum and a slow lingering retreat which lasted through the Kimmeridgian. These seem to have been worldwide movements. The table shows the interregional correlations. These are gross, of course, but they show the essential contemporaneity of the four main marine inundations of different regions during the Jurassic. The gaps in the table mark periods during which the lands were either emergent or lapped only by minor spreadings of the sea.

TABLE OF CORRELATIONS

Faunas	W. North America	Alaska	E. Greenland	India	Europe
Cardioceratan to Rasenian	Sundance, Fernie (*pars.*), Mariposa, etc.	Naknek formation	Aucella River beds	Katrol-Dhosa "group"	Kimmeridgian-Argovian
Proplanulitan, etc.	Ellis, Fernie (*pars.*), Mysterious Creek, Minabariet, Bicknell, etc.	Chinitna Shale	Beds at Vardekløft, etc.	Chari "group"	Callovian
Sonninian to Stephanoceratan	Lowest Fernie, Opuntia shale, Mormon sandstone, etc.	Tuxedni sandstone	Mt. Nathorst beds, etc.	Putchum "group"	Bajocian
Coroniceratan to Liparoceratan	Harbledown, etc.	Beds with "Arietites"			Charmouthian-Sinemurian

The submergences in different places were contemporaneous, not synchronous. But the apparently world-wide extent of these submergences is rather remarkable when consideration is had of the fact that the local history of each region is very different from that of the others. Which is the cause? Eustatic rise of the sea, or contemporaneous subsidence of the lands?

Paleoclimatology. Climatic correlations of the Jurassic faunas have been avoided because of the lack of sufficient new data. The older conclusions, voiced by Neumayr and others, still hold in the main. These undoubtedly require a critical

revision, but that will be possible only when we have considerably more positive knowledge of the faunas, their zoölogical relationships, their distribution, and their stratigraphic occurrence. This is prerequisite to an understanding of the faunal geography on which climatic interpretations may be based. At present, anything but the greatest caution is folly.

Peculiarities of the Jurassic Seas. Some other general conclusions may be drawn. The great thicknesses of the Jurassic—2000 to 6000 feet in the Rocky Mountains, 5000 feet in central British Columbia, 18,000 feet in western British Columbia, 6000 feet in northern California—show that a great amount of subsidence took place. The continuity of deposits shows that subsidence was fairly regular. This conclusion is confirmed by the lack of truncation of formations except at a very few places.

These conclusions, together with a consideration of the shapes of the long arm-like seaways, seem to show that during the Jurassic, the seas expanded, not by their own erosion of their margins, but largely by virtue of subsidences which induced them to expand in the way they did. Presumably, the general surface being low and flat, utterly unlike the modern North America, a comparatively slight diastrophic down-warping would cause a notable marine expansion. But, in a few places, such as the borders of the Intermontane Geanticline,* the Jurassic lies upon the bevelled edges of the Trias. Here, there may have been expansion by erosion, but the total amount of such expansion was very small.

In general, as we review the Paleozoic and Mesozoic formations, the frequent repetitions of coarse-grained sediment, the multitude of fossils, and various evidence regarding the habits of the organisms, lead to the belief that most epeiric seas were shallow. But, the Jurassic deposits of the Pacific margin are peculiar, and not at all typical of epeiric seas as judged by European and Appalachian standards. The predominance of volcanics and "black shales" is remarkable.

* For instance, Central Oregon and the Interior Plateau of British Columbia.

The distribution of fossils: densely crowded in a few thin layers separated by thousands of feet of barren beds, is a problem.

Since the subsidence was enormous, it was also rapid; perhaps more rapid in places than the deposition of sediment. Hence, there may have resulted some unusually deep inland seaways during the Jurassic. There is some evidence tending toward such a suggestion. The rarity of fossils might mean either rapid deposition or original scarcity of animal life. The latter seems more likely: dark mud bottoms, both deep and shallow, have notoriously poor faunas. Concentration of fossils in a few thin layers may then be the result of condensation of the deposits bearing them—the result of agitation of the bottom by wave-action which removes the mud and thus brings together the fossils of a considerable thickness of sediment. If this be true, certain of these Jurassic sea-bottoms lay most of the time below the limits of wave-action. But, we are far from knowing how sea-waves in Jurassic time compare with those of the present day. No conclusion is possible, of course; these things are still problems.

Continental Development. Even more remarkable than the peculiarities of the seas are the structural modifications of the continent. Along its eastern margin the continent seems to have lost much of its substance, whereas in the west it seems to have gained. During the Paleozoic, a great land-mass lay off what is now the eastern edge of the continent.[113] At no time did the Atlantic waters spread directly westward over this territory. But, during the Jurassic, the last and most important of the changes, whatever they were, transpired to make possible a direct westward inundation in the Cretaceous.

In the west, throughout the Cordilleran belt from the Pacific to the border of Laurentia, the sedimentation of the Paleozoic and Triassic built a great cumulus of deposits, buttressed here and there by volcanics, but otherwise without secondary structures. The subsidence of this belt, called by Schuchert and others the Cordilleran "Geosyncline," was

[113] J. Barrell: *Am. Jour. Sci.*, Vol. 37, 1914.

broad and general in character until the Upper Triassic. Then, from an unknown origin, the Cordilleran Intermontane Geanticline arose,* a broad, folded arch. And thus was inaugurated the diastrophic compression of the cumulus.

The original Cordilleran Geosyncline was now divided into two sequent geosynclines, the Rocky Mountain and the Pacific. The Jurassic history of the former is merely that of irregular subsidence: alternating deposition and erosion, with deposition in the ascendant. But the Pacific geosyncline was the scene of a restless conflict between diastrophism, vulcanism, and erosion. Sediments and volcanics accumulated to enormous thicknesses, only to be crumpled and stiffened along certain zones by mountain-makings, and bevelled by erosion. Finally, some of these mountain belts became rigidly braced by the solidification of vast igneous intrusions. Thus the Jurassic history of this region has been a breaking up of the old marginal geosyncline into several sequent troughs, separated by mountain ranges.

The process of transformation, thus begun during the Jurassic, was by no means complete by the end of that period. Subsequently, one by one, the various elements including sequent geosynclines, synclinoria, and even geanticlines, have yielded to the compressive stresses. Finally, with the Laramide Revolution the compression became general, and the first "Cordillera" was formed. The influence of Jurassic peculiarities was still felt, for in this and subsequent mountain-makings, the nature of the Jurassic materials in the cumulus gave a peculiar character to the deformation—fracturing became dominant; plication, subordinate.

Thus, this western region has been transformed. Out of the ancient marginal "geosyncline" (a dominantly sinking zone which, however, belonged neither to continent nor ocean) the modern Cordillera (an intensely compressed, dominantly positive zone) has arisen. And this has become welded to the

* Perhaps it is more appropriate to refer to the beginnings of this structure as the Sonoran Geanticline.

nucleus, Laurentia, thus extending by so much the margin of the kratogen.[114]

These considerations seem to show that continents and oceans are permanent only in a relative sense. Their boundaries are not absolutely permanent as some have maintained, any more than they are ephemeral as others have thought. Both continents and oceans appear to have *grown* at the expense of each other. Their growth is slow: just as they are the largest, so they are the longest-lived features of a world in which everything changes, though in different degrees.

Acknowledgments

The author wishes to thank collectively the many friends who have so kindly aided this research by contributing information or collections. The kindness of both American and foreign correspondents in sending copies of their works is especially appreciated.

Dependence upon Buckman's Results. The rocks of the American Jurassic are too broken by diastrophism, too discontinuous, too poor in fossils, to serve for the working out of the succession except by correlation of their parts by comparison with a standard chronology. Buckman's biologic chronology provides such a standard.[115, 116, 71]

In view of the dependence of those working in the North American Jurassic on Buckman's results, it seems proper to offer some short explanation of his system of chronologizing geologic time. Buckman started from the standpoint of the principle of the essential independence of faunal and sedimentary history. Formations are records of the discontinuous and irregular process of sedimentation. The appearance of regularity and continuity which characterizes sedimentary structures is, judged by paleontological evidence, an illusion. Neither continuity of the structures nor of lithology is any criterion of continuous deposition. Gaps in the sedimentary

[114] T. Mellard Reade: *The Evolution of Earth Structure*, London, 1903. Compare the whole argument presented here with Reade's suggestions in his chapter 3.

[115] S. S. Buckman: *Q. J. G. S.*, Vol. 54, 1898.

[116] S. S. Buckman: Monogr. Pal. Soc., 1887–1907.

[71] S. S. Buckman: *Type Ammonites*, 1909–1928.

record may not necessarily reveal themselves. Moreover, they never reveal the length of time they represent. Nor is the thickness even roughly proportional to the time taken to form the deposit. These considerations show that though we base geologic history on the evidence of the sediments, we may not base the *chronology* of that history on such evidence.

Faunas, on the other hand, are disconnected records of stages in the continuous and orderly, though not necessarily even, process of organic evolution. Faunas indicate their relationships to one another in several ways: by their stratigraphic occurrence; by their physical condition; and by their composition: with regard to general similarity or the lack of it, progressive evolution of individual elements, changes in faunal equilibrium. Each fauna, forming, as it does, a *certain* stage in a continuous process of organic change, represents a *certain* period in the continuous passage of time.

The chief difficulty is that faunas are just as discontinous in themselves as the deposits which contain them. The order of succession of several faunas in a single section may be readily obtained. And, by analysis of a sufficiently large number of single successions, the relative position of *any* two faunas may be learned. Thus, all the faunas may be placed correctly in succession. This artificially brought together succession of faunas is an approximation to the result of continuous deposition. It is the framework of a chronology in which each fauna represents a date. No single section ever gave such a complete result.

All this sounds simple enough, and furthermore, much of it has been understood since the days of William Smith. Nearly all of it has been understood since the work of Henry Shaler Williams. Yet, very little has been accomplished toward chronologizing geologic time. The subdivision of the early Paleozoic periods and the Triassic is still pretty coarse. The Carboniferous is notoriously confused. In the Jurassic, the work of Buckman has produced a detailed biological chronology of the entire period. Latterly, Spath has extended these principles to the Cretaceous.

Having obtained the chronology, at least in its rough beginnings, Buckman has named each date after a genus or species which, in the unit of time for which that "date" stands, either made its only appearance or attained its acme of prosperity. Further, he has grouped these units of time, or hemeræ, of which there are about 400 in the Jurassic period, into "ages," of which there are 50 in the same period. Each age is named after the maximum development of either a family or a generic group. The rapid migration and wide distribution of ammonites makes possible sharp correlation of the faunas of various parts of the world.

Special Acknowledgments. A word of acknowledgment must be made of the keenly critical paleontological and stratigraphical work on Canadian Jurassic faunas done in late years by F. H. McLearn. His labours have resolved some long-standing puzzles in that territory.

Also, the excellent critical summary of Alaskan Mesozoic stratigraphy, by G. C. Martin,[27] has saved the author many a long search after obscure information. Martin's clear understanding of the subject, and his perfection of completeness, make the work an object for admiration. However, Martin has made no attempt to correct fossil names or to indicate anomalies of fossil occurrence. In using the work, it is well to beware of this.

Nothing comparable to this has ever been done for Canadian and American Mesozoic paleontology and stratigraphy. As Professor E. Dacqué has well said, there is urgent need for such a general revision.[117] The only modern attempt, Goranson's work, is too incomplete and too flagrantly inaccurate to be of any value.

There are two useful fossil checklists. Boyle's [118] lists the species described between 1820 and 1892. Whitney's lists those described between 1892 and 1922.[119] Both of these works are accurate, though neither is perfectly complete.

[27] G. C. Martin: U. S. G. S., Bull. 776, 1926.
[117] E. Dacque: *Geol. Rundschau*, 2, 1911.
[118] C. B. Boyle: U. S. G. S., Bull. 102, 1893.
[119] F. L. Whitney: *Bull. Amer. Paleont.*, Vol. 12, 1928.

Finally, the writer has drawn many an inspiration from Charles Schuchert's teaching and from his brilliant memoir on American paleogeography. Also, he owes much to the example set by J. P. Smith's masterly investigation of the West American Trias. It was he who directed the writer's first, somewhat frantic, attacks on the American Jurassic. It is, therefore, especially a pleasure to acknowledge the personal inspiration and encouragement received from Professor Smith while one of his students at Stanford University. The writer dedicates his work to these, his two masters.

Interpretation of the Paleogeographic Maps

Map-making Technique. Fourteen maps are used to illustrate the geographic development of the continent from the beginning of the Jurassic until the beginning of the Cretaceous. Hitherto, a common practice has been to represent paleogeography by composite maps, that is, each map covers a considerable period of time; and consequently, comprises a number of successive, different geographic settings. All of Arldt's, Willis', and most of Schuchert's maps are of this nature. But it is a confused method of representation: an effect analogous to the printing of several pictures on one plate. Plainly, it is undesirable. On this account, each map presented herewith, has been made, as far as possible, to represent a unit of geologic time—a single hemera. Furthermore, the maps represent the culminations of geographic changes—the maxima of continental uplift, and of marine expansion, the maxima of diastrophic and volcanic activity.

The paleogeography is plotted on an azimuthal projection of present day North America. It must be recognized clearly that this is a distorted base, for the shape of the continent has been altered considerably by diastrophism since the Jurassic. Hence, the maps convey at best a distorted picture of Jurassic geography. They have, however, one advantage in maintaining the connection between the interpretation and the evidence.

This problem of distortion is a serious one: it can not be

neglected. Since the distortion is the result of foreshortening which varies endlessly in degree and direction from place to place, solution seems almost beyond hope. However, it seems possible that an approximate solution might be reached by plotting as many calculations of local foreshortening as possible on a base map. These might then be connected into *areas of equal foreshortening*. These areas might then, by a geometrical construction, be stretched to their original span.

The problem of determining local foreshortening is itself a difficult and a neglected one. However, serious attempts have been made to deal with it,[120, 121, 122, 63] and, moreover, these have been reasonably successful: their results inspire confidence. This is, to a large extent, because the attempts were undertaken in favourable localities. In most parts of North America, owing to the backwardness of public geological survey work, information available is insufficient. In many places, such as large areas of post-Jurassic plutonics or of Cenozoic lavas, it is impossible to make any calculation.

These difficulties have proved so formidable, that the main problem of smoothing out all the post-Jurassic foreshortening does not admit at present of a really satisfactory solution. However, some rough results have been obtained. These seem to show that the Cordillera has been narrowed in all latitudes by nearly the same amount—280 miles: the compression being closer in some cross-sections than in others. The result has been checked in only one section by a detailed examination. This runs from Nitinat, Vancouver Island, to the "Gap" of Ghost River, Alberta, and is 400 miles long. The post-Jurassic shortening is 280 miles or 38 per cent.*

In view of the uncertainties entering into the problem, it has seemed undesirable to publish the author's rough approximation to an undistorted base map. However, it may be

[120] R. G. McConnell: G. S. C., Ann. Rept., 2, 1887.
[121] R. T. Chamberlin: *Jour. Geol.*, 18, 1910.
[122] R. T. Chamberlin: *Jour. Geol.*, 27, 1919.
[63] G. R. Mansfield: U. S. G. S., Prof. Paper 152, 1927.
* The one really doubtful element which may vitiate the calculation is the pre-Cambrian area between Shuswap Lake and the Rocky Mountain Trench.

worth while to mention one striking difference brought out by plotting paleogeography on the "undistorted base." Jurosonora appears almost twice as large as it does on the ordinary base: in fact it has continental proportions. This is not really surprising. It accords well with the area that might be expected after consideration of the great volume of rock waste derived by erosion from Jurosonora, and now partly incorporated in certain of the coarse clastic deposits of the Colorado Plateau and surrounding regions.

This example, alone, shows the need for the appreciation of diastrophic compression of regions in paleogeographic research. Until reliable quantitative results are obtained, no one can say how much unappreciated foreshortening distorts our pictures of ancient geography. Though the final solution is beyond the scope of the author's immediate research, and attack upon it must be deferred; yet what has been done so far, in the attempt to determine at least the order of inaccuracy in paleogeographic maps, is a step, however faltering, in the right direction.

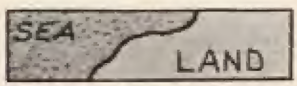
Summary of Paleogeography. Map number 1 represents the ancestral North American land mass of the beginning of the Jurassic Period. The continent is notably emergent. There are three so-called positive segments. These are outlined for convenience on this map, though their identity, as such, is seen only from a comparison of all the maps of the period. The first of these is the central nucleus or shield, Jurolaurentia. The second is the Sonoran geanticline, Jurosonora. The third, the Alaskan geanticline, Juroberingia. Between the geanticlines and the shield there is the beginning of a structural depression which, during the Jurassic, became the Rocky Mountain geosyncline. To the west of all of these lies the broad continental shelf. The southern and eastern boundaries of the continent are unknown.

Map number 2 represents the early acme in the Microderoceratan age of the first Lower Jurassic marine invasion—a shelf sea. The time is that marked by the genus *Arniotites.* In Central Mexico, there is a geosynclinal sea which probably

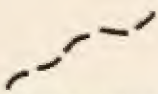
extended over most of Central America. At least the Pacific shelf sea lingered with minor changes through most of the Lower Jurassic.

Map number 3 shows a much later stage of the Lias seas. The date is about Grammoceratan, *thouarsense*, which includes most of the *Parapecten* "*acutiplicatus*" occurrences. There are no really significant changes since the time represented by the last map except a general shrinking of the epicontinental

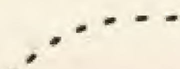
LEGEND OF THE MAPS



Shorelines.



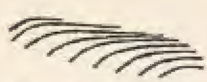
Shorelines of doubtful location.



Boundaries of continental nuclei.

Areas of continental deposits.

Volcanoes.



Tectonic mountains.

Maps are left blank where the information is insufficient, *e.g.*, east of Labrador and in the Antillean region.

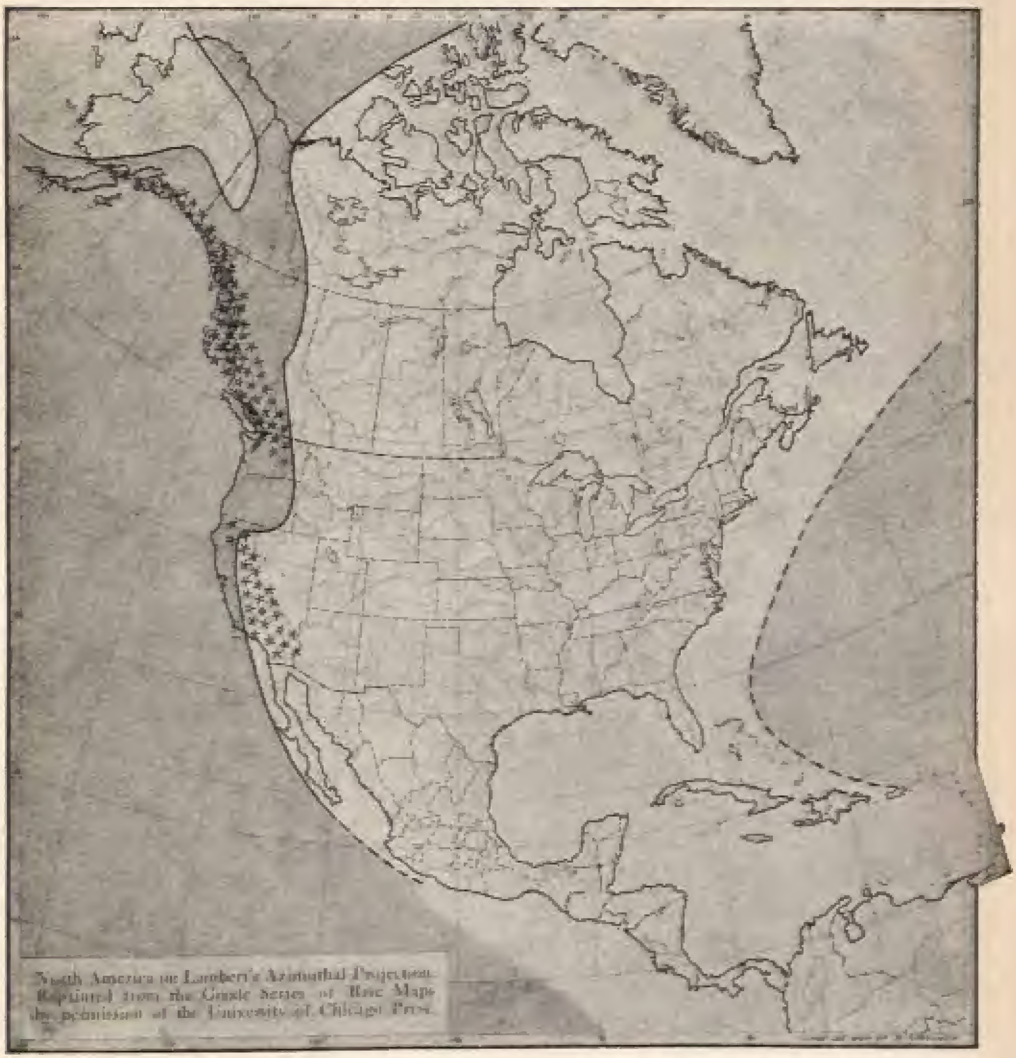
Place names are printed on the first, fifth, and fourteenth maps.

All the maps are on a scale of 1070 miles to 1 inch.

MAP NO. 1.—Beginning of the Jurassic Period.

MAP NO. 2.—Early Lower Jurassic—Microderoceratan, *Arniotites*.

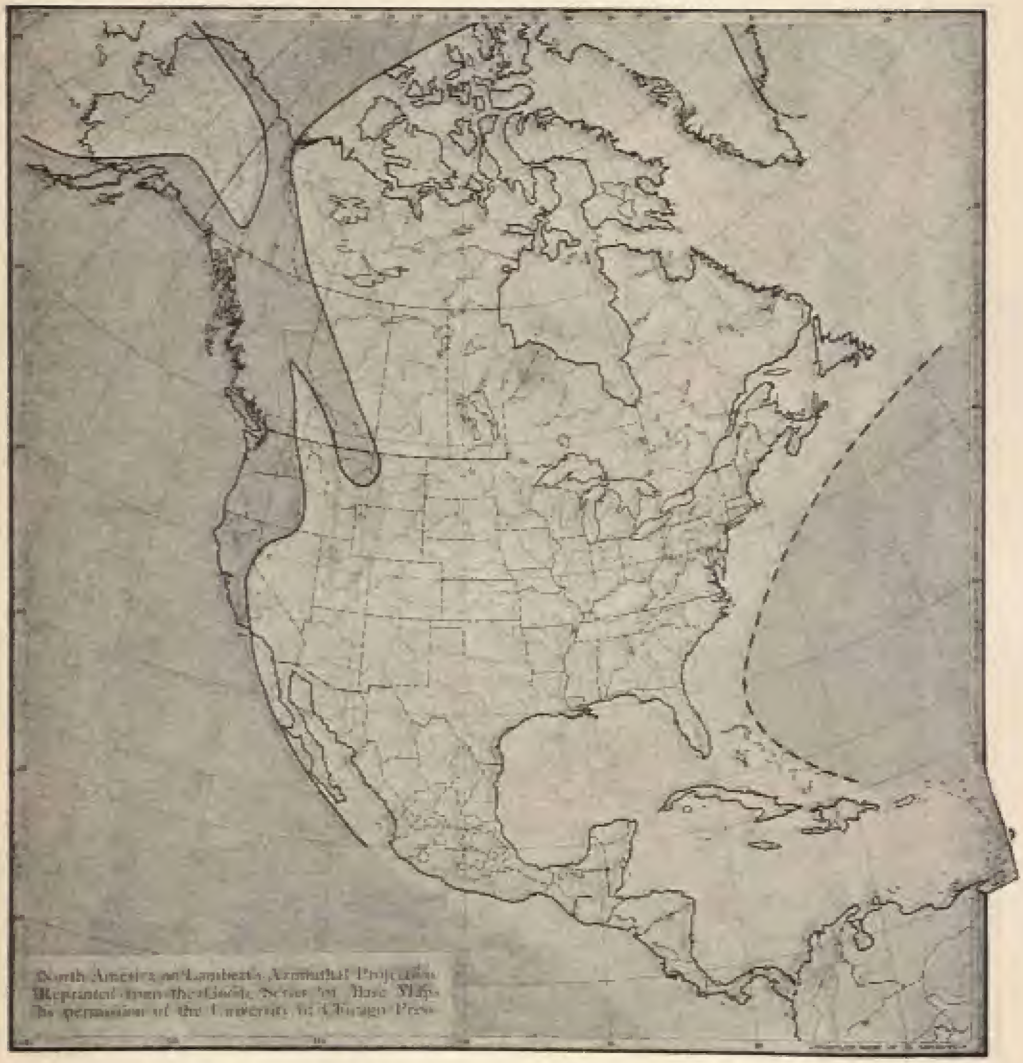
Map no. 3.—Late Lower Jurassic—Grammoceratan, *thouarsense*.



MAP NO. 4.—Early Middle Jurassic—Sonninian, "*discita*."

MAP NO. 5.—Early Middle Jurassic—Sonninian, *alsaticus*.

MAP NO. 6.—Late Middle Jurassic.



Map no. 7.—Early Upper Jurassic—Proplanulitan, *Metacephalites*.

MAP NO. 8.—Early Upper Jurassic—late Callovian, "*Trigonia*" *plumasensis-montanaensis*.

MAP NO. 9.—Early Upper Jurassic—Divesian, "*Quenst.*" *collieri*.

MAP NO. 10.—Mid Upper Jurassic—Cardioceratan, *Goliathiceras*.

MAP NO. 11.—Mid Upper Jurassic—Rasenian, *Amœboceras dubium*.

MAP NO. 12.—Late Upper Jurassic—Berriasellidan, *Berriasella* cf. *calisto*.

MAP NO. 13.—End of the Jurassic Period.

Map no. 14.—Early Lower Cretaceous—Neocomian, *crassicollis*.

seas. The marginal sea of the Arctic islands shows a gain.*

Map number 4 shows the early Middle Jurassic vulcanism and the seas restricted thereby. The vulcanism may have started as early as the Dumortierian age. It resulted in the formation of two great volcanic land masses—Juroporphyria and Jurocalifornia. The southern volcanic area makes a notable modification in the shoreline of the Californian sea. The northern volcanic area forms a great fringing island mass with a considerable sea to the east of it. Date: Sonninian, "*discitæ*."

Map number 5 represents the time of cessation of volcanic eruptions, from Sonninian, *sauzei*, to Stepheoceratan, *Epalxites*, and possibly longer. The exact date of the map is Sonninian, *alsaticus*. This interruption of the vulcanism is attended by a spread of the seas. The Pacific margin of the continent is flooded widely, and the seas penetrate for the first time into the Rocky Mountain geosyncline. The much extended northern end of the Intermontane Geanticline now stands out in relief as the land mass Jurozephyria between two seaways. Juroporphyria was presumably completely inundated, but Jurocalifornia only partly so: its emergent portion modifying the shape of the shoreline of that region.

Map number 6 representing (unfortunately) no exact time, but in general, the later Middle Jurassic, shows the second main vulcanism of the period. It is presumed that this was accompanied by a general retreat of the seas, for no fossiliferous deposits of this age are known in North America.

Map number 7 shows the early Upper Jurassic (Callovian) marine invasion which followed the cessation of Middle Jurassic volcanic activity. The date represented is about the middle of the Proplanulitan age, marked by the existence of such genera as *Metacephalites*, *Lilloettia*, etc. A broad sea covers the Pacific border and the Rocky Mountain geosyncline which were broadly connected with each other and with the

* Since this was written, Jaworski's paper, Abhandl. Schweiz. palaeont. Gesell., Bd. 48, 1929, has come to hand; and it seems to show that the mid Lias shorelines of the west side of Jurosonora should be about 200 miles to the northeast of their position on the map.

Arctic. Jurozephyria appears to be a permanent land mass. So, also, is the southern part of Jurocalifornia which has become welded to Jurosonora.

Map number 8 shows the overwhelming of a part of the early Upper Jurassic seas by vulcanism. The extent of this vulcanism is not certainly known. Probably that shown on the map is not sufficiently extensive. Late Callovian.

Map number 9 represents the Agassiz orogeny of Divesian time, and the conjectural retreat of the seas. The extent of this orogeny is very imperfectly known, but there is no doubt that tectonic mountains were formed along much of the margin of the continent. This, in turn, made a geosyncline of the belt between these mountains and the Cordilleran Intermontane Geanticline. This is the Pacific Geosyncline. During the orogeny, a sea of considerable extent seems to have persisted in the Rocky Mountain geosyncline.

Map number 10 represents the culmination in Argovian time (Cardioceratan, *Goliathiceras*) of the second great Upper Jurassic marine expansion. The time of widest extent of this sea coincides with the Cardioceratan age. It is presumed that Juroporphyria stood above water after the Agassiz orogeny. It is not known whether any of the California coastal region did likewise.

Map number 11 represents a stage in the retreat of the Upper Jurassic seas—Kimmeridgian epoch, Rasenian age. The Rocky Mountain Geosyncline is drained except for its northernmost part, but seas linger in the Pacific Geosyncline and on the shelf. There was an extensive sea in Mexico, reaching southern Texas, and covering much of the Central American-Caribbean region.

Map number 12 represents either the last stage in the retreat of the Upper Jurassic seas, or an independent late Jurassic inundation of Tithonian age: it is uncertain whether any disturbance or emergence separated this from the Kimmeridgian seas. The Mexican sea is from Proniceratan age to Berriasellidan.

Map number 13 represents the continent at the end of the

Jurassic Period. It shows the Jurasside (or Nevadian) mountain ranges which rose along the Pacific border. The continent is generally emergent. The shorelines shown in the Gulf and Caribbean are highly conjectural: they simply accord with the popular opinion among geologists, there being insufficient evidence for an independent conclusion. In the interior of the continent, two areas of continental sedimentation are shown.

Map number 14 represents the marine invasions of the early Cretaceous. These reached an early maximum in the Neocomian. The date represented on the map is that of *Buchia crassicollis*. Presumably, continental sedimentation continued in the interior. The Cordilleran Intermontane Geanticline extends from Mexico almost to the Arctic. At this stage, it is effective as a barrier between the troughs, only, because the north end of the Rocky Mountain geosyncline is emergent. This axis continued to grow during the Lower Cretaceous until finally the Oregonian orogeny (Middle Cretaceous) welded it to Beringia and thus cut off communication between Arctic and Pacific. As a result, East Indian faunas appear immediately in Japan and western North America; and simultaneously, Russian and Arctic immigrations cease.

The true Pacific shelf is much reduced. It is not known whether the mountains were yet crowded into the ocean, as the Cordillera is now. The supposedly mountainous islands of Juroporphyria and Mendocinia suggest that they may have been.

An extraordinary though undepicted growth was taking place in the Rocky Mountain Geosyncline, as a result of which, this trough became, one long, continuous sea from Caribbean to Arctic in the Upper Cretaceous.

Bibliography

Lack of space precludes giving a complete bibliography of the North American Jurassic.* In the following list only

* Such a thing is needed. It might appear appropriately in connection with a thoroughgoing summary and revision of stratigraphy and paleontology.

those contributions are included to which direct reference is made in this memoir; and this, of course, limits it to those which have a definite bearing on the arguments brought forward. Naturally many important works dealing with local geology are left out. These have of course been consulted, but for the sake of necessary brevity are not specially referred to.

99. W. W. Atwood: "Geology and Mineral Resources of Parts of the Alaska Peninsula," U. S. Geological Survey, Bulletin 467, 1911.

23. J. A. Bancroft: "Geology of the Coast and Islands between the Strait of Georgia and Queen Charlotte Sound, B. C.," Geological Survey, Canada, Memoir 23, 1913.

113. J. Barrell: "The Upper Devonian Delta of the Appalachian Geosyncline," *American Journal of Science*, Vol. 37, 1914.

109. G. F. Becker: "Notes on the Stratigraphy of California," U. S. Geological Survey, Bulletin 19, 1885.

98. E. Blackwelder: "A Summary of the Orogenic Epochs in the Geologic History of North America," *Journal of Geology*, Vol. 22, 1914.

88. E. Böse: "Vestiges of an Ancient Continent in Northeast Mexico," *American Journal of Science*, 5th Series, Vol. 6, 1923.

112. A. Bowman: "Report on the Geology of the Mining District of Cariboo, British Columbia," Geological Survey, Canada, Annual Report, Vol. 3, 1888.

118. C. B. Boyle: "A Catalogue and Bibliography of North American Mesozoic Invertebrata," U. S. Geological Survey, Bulletin 102, 1893.

110. J. C. Branner, J. F. Newsom and R. Arnold: "Description of the Santa Cruz Quadrangle, California," U. S. Geological Survey, Folio 163, 1909.

9. E. B. Branson: "Triassic-Jurassic 'Red Beds' of the Rocky Mountain Region," *Journal of Geology*, Vol. 35, 1927.

33. A. H. Brooks: "The Mount McKinley Region, Alaska," U. S. Geological Survey, Professional Paper 70, 1911.

102. A. H. Brooks and E. M. Kindle: "Paleozoic and Associated Rocks of the Upper Yukon, Alaska," Geological Society of America, Bulletin, Vol. 19, 1908.

116. S. S. Buckman: "Monograph of the Inferior Oolite Ammonites," *Palæontographical Society*, 1887–1907.

115. S. S. Buckman: "On the Grouping of Some Divisions of So-called 'Jurassic' Time," *Quarterly Journal of the Geological Society* (London), Vol. 54, 1898.

71. S. S. Buckman: *Type Ammonites*, 7 vols., London, 1909–1930.

60. S. S. Buckman: "Jurassic Ammonoidea. Mesozoic Palæontology of Blairmore Region, Alberta," National Museum of Canada, Bulletin 58, 1929.

85. C. Burckhardt: "La Faune Jurassique de Mazapil," Instituto Geologico de Mexico, Boletin 23, 1906.

83. C. Burckhardt: "Neue Untersuchungen über Jura und Kreide in Mexiko," *Centralblatt für Mineralogie, Geologie, und Paläontologie*, 1910.

86. C. Burckhardt: "Faunes Jurassiques et Cretaciques de San Pedro del Gallo," Instituto Geologico de Mexico, Boletin 29, 1912.

87. C. Burckhardt: "Faunas Jurasicas de Symon (Zacatecas)," Instituto Geologico de Mexico, Boletin 33, 1919.

21. C. E. Cairnes: "Coquihalla Area, British Columbia," Geological Survey, Canada, Memoir 139, 1924.

101. S. R. Capps: "The Chisana-White River District," Alaska, U. S. Geological Survey, Bulletin 630, 1916.

121. R. T. Chamberlin: "The Appalachian Folds of Central Pennsylvania," *Journal of Geology*, Vol. 18, 1910.
122. R. T. Chamberlin: "The Building of the Colorado Rockies," *Journal of Geology*, Vol. 27, 1919.
7. R. T. Chamberlin: "The Significance of the Framework of the Continents," *Journal of Geology*, Vol. 32, 1924.
104. C. H. Clapp: "Sooke and Duncan Map-Areas, Vancouver Island, Geological Survey, Canada, Memoir 96, 1917.
46. W. B. Clark and M. W. Twitchell: "The Mesozoic and Cenozoic Echinodermata of the United States," U. S. Geological Survey, Monograph 54, 1915.
82. F. W. Cragin: "Paleontology of the Malone Jurassic formation of Texas," U. S. Geological Survey, Bulletin 266, 1905.
16. C. H. Crickmay: "A Note on Two of Hyatt's Liassic Ammonites," California Academy of Sciences, Proc., 4th Ser., Vol. 14 (1925).
68. C. H. Crickmay: "The Geology and Paleontology of the Harrison Lake District, British Columbia, together with a General Review of the Jurassic Faunas and Stratigraphy of Western North America," Abstracts of Dissertations, Stanford University, 1924–26, 1927.
13. C. H. Crickmay: "The Stratigraphy of Parson Bay, British Columbia," University of California, Publications in Geology, Vol. 18, 1928.
2. C. H. Crickmay: "The Jurassic Rocks of Ashcroft, British Columbia," University of California, Publications in Geology, Vol. 19, 1930.
43. C. H. Crickmay: "Fossils from Harrison Lake Area, British Columbia," National Museum, Canada, Bulletin 63, 1930.
117. E. Dacque: "Die Stratigraphie des marinen Jura an den Rändern des Pazifischen Ozeans," *Geologische Rundschau*, Bd. 2, 1911.
20. R. A. Daly: "Geology of the North American Cordillera at the Forty-ninth Parallel, Geological Survey, Canada, Memoir 38, 1912.
75. C. H. Davis: "New Species from the Santa Lucia Mountains, Cal., with a Discussion of the Jurassic Age of the Slate at Slate Springs," *Journal of Geology*, 21, 1913.
66. E. F. Davis: "The Franciscan Sandstone," University of California Publications Geology, Vol. 11, 1918.
26. G. M. Dawson: "Report on the Queen Charlotte Islands," Geological Survey, Canada, Report of Progress for 1878–79, 1880.
22. G. M. Dawson: "Report on a Geological Examination of the Northern Part of Vancouver Island and Adjacent Coasts," Geological Survey, Canada, Annual Reports, Vol. 2, 1887.
106. J. W. Dawson: "On the Mesozoic Floras of the Rocky Mountain Region of Canada," Royal Society of Canada, Proceedings & Transactions, Vol. 3, 1886.
45. J. S. Diller: "Description of the Redding Quadrangle," U. S. Geological Survey, Redding Folio (138), 1906.
80. J. S. Diller: "The Mesozoic Sediments of Southwestern Oregon," *American Journal of Science*, 4th Series, Vol. 23, 1907.
56. J. S. Diller: "Geology of the Taylorsville Region," U. S. Geological Survey, Bulletin 353, 1908.
94. J. S. Diller and G. F. Kay: "Description of the Riddle Quadrangle," U. S. Geological Survey, Folio 218, 1924.
92. J. S. Diller and T. W. Stanton: "The Shasta-Chico Series," Geological Society of America, Bulletin, Vol. 5, 1894.
58. E. von Eichwald: "Geognostisch-paleontologische Bemerkungen über die Halbinsel Mangischlak und die Aleutischen Inseln," Buchdruckerei der kaiserliche Akademie der Wissenschaften, St. Petersburg, 1871.
111. H. W. Fairbanks: "The Validity of the So-called Wallala Beds as a Division of the California Cretaceous," *American Journal of Science*, Vol. 45, 1893.

93. H. W. Fairbanks: "Description of the San Luis quadrangle," U. S. Geological Survey, Folio 101, 1904.

47. J. Felix and H. Lenk: "Beiträge zur Geologie und Palæontologie der Republik Mexiko," 3 parts, Leipzig, 1890, 1893–99; Stuttgart, 1891.

15. W. M. Gabb: "Descriptions of Some Secondary Fossils from the Pacific States," *American Journal of Conchology*, Vol. 5, 1869.

57. C. Grewingk: "Beiträg zur Kenntnis der orographischen und geognostischen Beschaffenheit der nordwest Küste Amerikas mit den anliegenden Inseln," *Russisch-kaiserliche mineralogische Gesellschaft zu St. Peterburg. Verhandlungen 1848–49*, 1850.

5. E. Haug: "Les geosynclinaux et les aires continentales," *Bulletin de la société géologique*, Ser. 3, tome 28, 1900.

30. L. G. Hertlein: "New Species of Marine fossil Mollusca from Western North America," Southern California Academy of Sciences, Bulletin, Vol. 24, 1925.

28. A. Hyatt: "Jura and Trias at Taylorsville, California," Geological Society of America, Bulletin, Vol. 3, 1892.

18. A. Hyatt: "Trias and Jura in the Western States," Geological Society of America, Bulletin, Vol. 5, 1894.

107. J. Keele: "Clay and Shale Deposits of the Abitibi and Mattagami Rivers," Ontario Bureau of Mines, Annual Report, Vol. 29, pt. 2, 1920.

84. F. L. Kitchin: "The So-called Malone Jurassic Formation in Texas," *Geological Magazine*, Vol. 63, 1926.

37. A. Knopf: "A Geologic Reconnaissance of the Inyo Range and the Eastern Slope of the Southern Sierra Nevada, Cal.; with a Section on the Stratigraphy of the Inyo Range, by Edwin Kirk," U. S. Geological Survey, Professional Paper 110, 1918.

56. A. Knopf and S. Paige: "Stratigraphic Succession in the Region Northeast of Cook Inlet, Alaska," Geological Society of America, Bulletin, Vol. 18, 1907.

95. F. H. Knowlton: "The Jurassic Age of the 'Jurassic Flora of Oregon,'" *American Journal of Science*, 30, 1910.

90. A. C. Lawson: "The Cordilleran Mesozoic Revolution," *Journal of Geology*, Vol. 1, 1893.

74. A. C. Lawson: "Description of the San Francisco District; Tamalpais, San Francisco, Concord, San Mateo and Haywards Quadrangles," U. S. Geological Survey, San Francisco Folio, No. 193, 1914.

11. W. T. Lee: "Early Mesozoic Physiography of the Southern Rocky Mountains," Smithsonian Miscellaneous Collections, Vol. 69, No. 4, 1918.

48. E. de K. Leffingwell: "The Canning River Region, Northern Alaska," U. S. Geological Survey, Professional Paper 109, 1919.

1. C. Lebling: "Tektonische Forschungen in den Appalachen," *Geologische Rundschau*, 5, 1915.

73. W. N. Logan: "A North American Epicontinental Sea of Jurassic Age," *Journal of Geology*, Vol. 8, 1900.

8. C. R. Longwell: "Geology of the Muddy Mountains, Nevada," Bulletin 798, U. S. Geological Survey, 1928.

76. W. S. McCann: "Geology and Mineral Deposits of the Bridge River Map-Area, British Columbia," Geological Survey, Canada, Memoir 130, 1922.

120. R. G. McConnell: "Report on the Geological Structure of a Portion of the Rocky Mountains," Geological Survey, Canada, Annual Report, Vol. 2, 1887.

61. J. D. MacKenzie: "Geology of Graham Island, British Columbia," Geological Survey, Canada, Memoir 88, 1916.

54. F. H. McLearn: "Jurassic and Cretaceous, Crowsnest Pass, Alberta," Geological Survey, Canada, Summary Report for 1915, 1916.

50. F. H. McLearn: "Peace River Canyon Coal Area, B. C.," Geological Survey, Canada, Summary Report for 1922, 1923.

52. F. H. McLearn: "New Pelecypods from the Fernie Formation of the Alberta Jurassic," *Royal Society of Canada, Transactions*, 3d, Ser., Vol. 18, 1924.
42. F. H. McLearn: "New Jurassic Species from the Hazelton Group of British Columbia," Geological Survey, Canada, Bulletin 44, 1926.
31. F. H. McLearn: "Some Canadian Jurassic Faunas," *Royal Society of Canada, Transactions*, 3d Ser., Vol. 21, 1927.
108. F. H. McLearn: "The Mesozoic and Pleistocene Deposits of the Lower Missinaibi, Opazatika, and Mattagami Rivers, Ontario," Geological Survey, Canada, Summary Report for 1926, 1927.
51. F. H. McLearn: "New Jurassic Ammonoidea from the Fernie Formation, Alberta," Geological Survey, Canada, Bulletin 49, 1928.
65. F. H. McLearn: "Stratigraphic Palæontology. In Mesozoic Palæontology of Blairmore Region, Alberta," National Museum of Canada, Bulletin 58, 1929.
41. F. H. McLearn: "Contributions to the Stratigraphy and Palæontology of Skidegate Inlet, Queen Charlotte Islands, B. C.," National Museum, Canada, Bulletin 54, 1929.
63. G. R. Mansfield: "Geography, Geology, and Mineral Resources of Part of Southeastern Idaho," U. S. Geological Survey, Professional Paper 152, 1927.
27. G. C. Martin: "The Mesozoic Stratigraphy of Alaska," U. S. Geological Survey, Bulletin 776, 1926.
40. G. C. Martin and F. J. Katz: "A Geologic Reconnaissance of the Iliamna Region, Alaska," U. S. Geological Survey, Bulletin 485, 1912.
39. G. C. Martin and F. J. Katz: "Geology and Coal Fields of the Lower Matanuska Valley, Alaska," U. S. Geological Survey, Bulletin 500, 1912.
29. F. B. Meek: "Descriptions of the Jurassic Fossils," *Palæontology of California*, Vol. 1, 1864.
81. F. B. Meek: "Descriptions of Fossils from the Auriferous Slates of California," *Geology of California*, Vol. 1, 1865.
53. F. B. Meek: "Paleontological Report," U. S. Geological Survey of the Territories (Hayden), 6th Annual Report, 1873.
100. W. C. Mendenhall: "A Reconnaissance from Resurrection Bay to the Tanana River, Alaska, in 1898," U. S. Geological Survey, 20th Annual Report, part 7, 1900.
79. F. H. Moffit and A. Knopf: "Mineral Resources of the Nabesna-White River District, Alaska," U. S. Geological Survey, Bulletin 417, 1910.
105. C. C. Mook: "A Study of the Morrison Formation," *New York Academy of Sciences, Annals*, Vol. 27, 1916.
55. M. Neumayr: "Geographische Verbreitung der Juraformation," *Kaiserlich-königliche Akademie der Wissenschaften*, Wien; Denkschriften, Bd. 50, 1885.
77. J. J. O'Neill: "Preliminary Report on the Economic Geology of Hazelton District, British Columbia," Geological Survey, Canada, Memoir 110, 1919.
34. S. Paige and A. Knopf: "Geologic Reconnaissance in the Matanuska and Talkeetna Basins, Alaska," U. S. Geological Survey, Bulletin 327, 1907.
56. S. Paige and A. Knopf: "Stratigraphic Succession in the Region Northeast of Cook Inlet, Alaska," Geological Society of America, Bulletin, Vol. 18, 1907.
96. A. P. Pavlov: "Enchaînement des Aucelles et Aucellines du crétacé russe," Société impériale des naturalistes de Moscou, Nouveau Mêmoires, tome 17, 1907.
59. J. F. Pompeckj: "Jura-Fossilien aus Alaska," *Russisch-kaiserliche mineralogische Gesellschaft zu St. Peterburg*, Verhandlungen, 1900.
114. T. Mellard Reade: *The Evolution of Earth Structure* (a book) London, 1903.
70. J. B. Reeside, Jr.: "Some American Jurassic Ammonites of the Genera Quenstedticeras, Cardioceras, and Amœboceras, Family Cardioceratidæ, U. S. Geological Survey, Professional Paper 118, 1919.

10. J. B. Reeside Jr.: "Triassic-Jurassic 'red beds' of the Rocky Mountain Region": a discussion, *Journal of Geology*, Vol. 37, 1929.
14. J. B. Reeside, Jr., and James Gilluly: "Sedimentary Rocks of the San Rafæl Swell and Some Adjacent Areas in Eastern Utah," U. S. Geological Survey, Professional Paper 150-D, 1928.
6. R. Ruedemann: "The Existence and Configuration of Precambrian Continents," New York State Museum, Bulletin 239–40, 1922.
103. S. J. Schofield and G. Hanson: "Geology and Ore Deposits of Salmon River District, British Columbia," Geological Survey, Canada, Memoir 132, 1922.
3. C. Schuchert: "Sites and Nature of the North American Geosynclines," Geological Society of America, Bulletin, Vol. 34, 1923.
4. C. Schuchert: "Paleogeography of North America," Geological Society of America, Bulletin, Vol. 20, 1910.
19. J. P. Smith: "The Geologic Formations of California, with Reconnaissance Geologic Map," California State Mining Bureau, Bulletin 72, 1916.
91. J. P. Smith: "Age of the Auriferous Slates of the Sierra Nevada," Geological Society of America, Bulletin, Vol. 5, 1894.
97. J. P. Smith: "Salient Events in the Geologic History of California," *Science*, N. S., Vol. 30, 1909.
78. W. R. Smith and A. A. Baker: "The Cold Bay-Chignik District," U. S. Geological Survey, Bulletin 755, 1925.
53. T. W. Stanton: "'Mesozoic Fossils.' In Geology of the Yellowstone National Park," U. S. Geological Survey, Monograph 32, pt. 2, 1899.
32. T. W. Stanton and G. C. Martin: "Mesozoic Section on Cook Inlet and Alaska Peninsula," Geological Society of America, Bulletin, Vol. 16, 1905.
12. H. W. Turner: "A Sketch of the Historical Geology of Esmeralda County, Nevada," *American Geologist*, Vol. 29, 1902.
72. C. A. White: "Report on the Geology of a Portion of Northwestern Colorado," U. S. Geological and Geographical Survey of the Territories (Hayden), Annual Report, Vol. 10, 1878.
62. C. A. White: "Contributions to Invertebrate Paleontology, No. 7; Jurassic Fossils from the Western Territories," U. S. Geological and Geographical Survey of the Territories (Hayden Survey), 12th Annual Report, 1883.
38. C. A. White: "On Invertebrate Fossils from the Pacific Coast," U. S. Geological Survey, Bulletin 51, 1889.
64. J. F. Whiteaves: "Notes on some Jurassic Fossils collected by Mr. G. M. Dawson in the Coast Range of British Columbia," Geological Survey, Canada, Report of Progress 1876–77, 1878.
24. J. F. Whiteaves: "Notes on Some Mesozoic Fossils from Various Localities on the Coast of British Columbia, for the Most Part Collected by Dr. G. M. Dawson in the Summer of 1885," Geological Survey, Canada, Annual Report for 1886, part B, Appendix 1, 1887.
49. J. F. Whiteaves: "On Some Cretaceous Fossils from British Columbia, the Northwest Territory and Manitoba," Geological Survey, Canada, Contributions to Canadian Palæontology, Vol. I, pt. 2, No. 4, 1889.
25. J. F. Whiteaves: "The Fossils of the Triassic Rocks of British Columbia," Geological Survey, Canada, Contributions to Canadian Palæontology, Vol. I, pt. 2, No. 3, 1889.
67. J. F. Whiteaves: "Description of a New Species of *Cardioceras* from the Crows Nest Coal Fields," *Ottawa Naturalist*, Vol. 17, 1903.
69. J. F. Whiteaves: "Description of a Canadian Species of Peltoceras," *Ottawa Naturalist*, Vol. 21, 1907.
44. J. F. Whiteaves: "Description of a New Species of Ammonite, of the Genus *Stephæoceras*, from Some Rocks of Presumably Jurassic Age in the Nicola Valley, B. C.," *Ottawa Naturalist*, Vol. 23, 1909.

89. J. D. Whitney: "The Auriferous Gravels of the Sierra Nevada of California," Museum of Comparative Zoology, Memoirs, Vol. 6, No. 1, 1879.

119. F. L. Whitney: "Bibliography and Index of North American Mesozoic Invertebrata," *Bulletins of American Paleontology*, Vol. 12, No. 48, 1928.

17. B. Willis: "Index to the Stratigraphy of North America," U. S. Geological Survey, Professional Paper 71, 1912.

35. C. W. Wright: "A Reconnaissance of Admiralty Island," U. S. Geological Survey, Bulletin 287, 1906.

—. E. Jaworski: "Eine Lias-Fauna aus Nordwest Mexiko," *Abhandl. Schweiz. Palaeont. Gesell.*, Bd. 48, 1929.

—. F. H. McLearn: "Notes on some Canadian Mesozoic Faunas," *Transactions, Royal Society of Canada*, 3d Series, Vol. 24, 1930.

RATIONALISED VERSUS UNRATIONALISED PRACTICAL ELECTROMAGNETIC UNITS

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(*Read April 25, 1931*)

Scope and Purpose.—Electromagnetic units were established in the centimeter-gram-second system (C.G.S.) by the British Association (B.A.) Committee in 1873; although an earlier B.A. Committee had advocated (1863–1867) the meter-gram-second system. The *ohm*, and *volt* were selected as working units at 10^9 and 10^8 C.G.S. magnetic units of resistance and electromotive force respectively, for the sake of convenience in magnitude and from these, the *ampere* (originally called the *weber*) was fixed as the working unit of current by the unitary relations of Ohm's law. Subsequent international electrical congresses adopted the *practical* or working series *ohm*, *volt*, *ampere*, *coulomb*, *farad*, *joule*, *watt*, and *henry* all in mutual unitary relation, originally as decimal derivatives of the fundamental C.G.S. magnetic system; but later as logical elements of the practical series.

Maxwell first showed [1] that the practical series formed a system analogous to the C.G.S. system; but with the earth *quadrant* of 10^9 cm substituted for the centimeter as unit length, and the *eleventh-gram* (10^{-11} gm) as unit mass, the *second* as time unit being common to both. The practical series thus became a part of the Q.E.S. magnetic system.

It was shown by Ascoli [2] in 1904; that if the numerical value attached to μ_0, the permeability of free space, might be accepted at some value other than unity; the above-mentioned eight international units of the practical series could be assigned to any one of an infinite number of systems; provided that the system length being 10^l cm and the system mass being

[1] Bibliography 1.

[2] Bibliography 9.

10^m gm, the exponents l and m satisfied the relation $2l+m=7$, a relation which depends upon the retention of the joule as 10^7 ergs. Of this infinite series the Q.E.S. system is the particular case in which $\mu_0 = 1$.

The only members of the infinite number of possible practical systems that seem to be worth discussing at present are:

	Length L	Mass M	Time T	Proposer	Title	Value of μ_0	
						rat	unrat
(*a*)	10^9 cm	10^{-11} gm	second	Maxwell	Q.E.S.	4π	1
(*b*)	10^2 cm	10^3 gm	"	Giorgi	M.K.S.	$4\pi \times 10^{-7}$	10^{-7}
(*c*)	10^0 cm	10^7 gm	"	Dellinger-Bennett	C.G.-S.S.	$4\pi \times 10^{-9}$	10^{-9}

It will be observed that the numerical value of space permeability μ_0 in each of these systems has two values differing in the ratio of 4π, according as the system is *rationalised* or *unrationalised*.

Rationalised and Unrationalised Systems of Units.—It was pointed out by Heaviside [1] that the C.G.S. system of units is unrationalised in the sense that its formulas contain the numerical constant 4π in the wrong places from a logical viewpoint. Thus, the C.G.S. capacitance of a prismatic or slab condenser introduces the 4π constant; whereas the capacitance of a concentric spherical condenser omits this constant. He proposed to rationalise and simplify the fundamental electromagnetic formulas both of the C.G.S. and Q.E.S. systems by altering the basic magnetic repulsion equation from

$$F = \frac{m^2}{\mu_0 r^2} \quad dynes \tag{1}$$

to

$$F = \frac{m^2}{4\pi\mu_0 r^2} \quad dynes, \tag{2}$$

where m is the value of each of two equal mutually repelling magnetic poles in free space and r is the separating distance in

[1] Bibliography 3, Chap. II, p. 116.

cm, keeping the value of μ_0 as unity. This method of rationalising would, however, involve a change in the numerical value of m in the ratio of $\sqrt{4\pi}$, for the same dynes repulsive force and the same distance r, which would lead in turn to changing the values of the standard international *ohm*, *volt*, *ampere*, etc., in some corresponding ratio. The quantity 4π not being a simple integer, the recalibration of all ohms, voltmeters, ammeters, etc. would have been a laborious and costly procedure and was unacceptable.

It was shown by several writers about the year 1895, that if the value of unity arbitrarily assigned to C.G.S. space permeability μ_0 were changed to 4π, equation (1) would become the equivalent of (2), and all the advantages of rationalisation could be secured in simplification of formulas, but without any change in the magnitudes of the practical units. A corresponding change in the numerical value of space permittivity k_0 would also be logically rendered necessary, and would extend the formula simplification into the electrostatic circuit.

It is doubtful whether it would be advantageous to rationalise the international C.G.S. system by changing its value of space permeability μ_0 from 1 to 4π. In its unrationalised and pristine classical form, the C.G.S. system has been used in so many text books of electromagnetics that it is not likely that any international support would be found in favor of the change. The Paris Electrical Congress of 1900 voted against it, and the I.E.C. Plenary meeting at Oslo in 1930 adopted the *gilbert*, or C.G.S. magnetic unit of magnetising force H, at the unrationalised value ($10/4\pi$ of an *ampere-turn*). When, however, we consider the practical series of units already internationally adopted—as recited in the opening paragraph of this paper, it may be noted that they are neutral, both as to their system and also as to rationalisation. They might belong equally well to any $2l + m$ system, and their system might be either rationalised or unrationalised. But any unit in this practical series which introduced length, area, volume or mass, would, when internationally adopted, logically assign the particular system to which the series belonged. Thus, if

the practical unit of electric field intensity E/L should be established as the *volt per quadrant*, *volt per meter* or *volt per cm*, the system would be included by inference. Similarly, for current density or amperes with respect to area I/S, or for energy per unit volume, W/V, or for charge per unit mass Q/M.

Again, any $2l + m$ system, that is, any electromagnetic system which assimilates the practical units already adopted, with a specification of the corresponding units of length and mass, might either be unrationalised like the basic C.G.S. system, or be rationalised and simplified in its formulas, by the proper corresponding choice of numerical values for μ_0 and k_0.

Questions at Issue in order to establish an International Complete Practical System of Electromagnetic Units.—In order to convert the existing series of eight practical units into a complete system corresponding in scope to the C.G.S. system, two questions must be settled.

(1) Shall the series be extended so as to form, when completed, a rationalised or unrationalised system?

(2) What shall be the nature of the system in regard to length and mass units (the time unit at the mean solar second not being in question). In other words, shall the system be the Q.E.S., the M.K.S. or the C.G.–S.S. system?

It is possible, since these questions are independent, to decide either question first. So far as practical magnetic units are concerned, the first question is probably more important, for initial settlement; because we cannot decide upon a practical unit for any of the series $\mathfrak{F}$, H, μ_0, $\mathcal{P}$, ν_0, $\mathfrak{R}$, $\mathfrak{J}$, $\mathfrak{M}$ and m, without settling the rationalisation question. Of these, the unit of $\mathfrak{F}$ (magnetromotive force) is a salient member. The only magnetic units which are independent of both questions are those of flux and inductance, Φ and L, the *pramaxwell* and *henry*, which have already been internationally adopted at Oslo and Chicago, respectively.

After decision may have been reached upon the rationalisation question, the decision as to the length and mass units

(L and M) of the practical system can be made more easily. There seem to be only three pairs of L and M at issue, namely (*a*) the quadrant and eleventh-gram, (*b*) the meter and kilogram, (*c*) the centimeter and gram-seven. It is doubtful, judging from the history of the subject, whether there is any real support for the (*a*) system; so that the choice appears to lie between (*b*) and (*c*).

It is the purpose of this paper to indicate the difference between the principal working formulas of the magnetic circuit with unrationalised and rationalised units, so as to offer material for aiding in the settlement of question (1). In each case the C.G.S. classical formula will be given, and the corresponding unrationalised and rationalised formulas in parallel columns. These will be given for the M.K.S. or (*b*) system; but if a reader desires to transfer the comparison to the (*a*) or (*c*) system, the transition can readily be found.

(*a*) *Magnetomotive Force* $\mathfrak{F}$, abb. *mmf.*

Equidimensional[1] formula $L^1M^{1/2}T^{-3/2}$ or $(1, \frac{1}{2}, -\frac{3}{2})$ or $\sqrt{P}$. C.G.S. unit the *gilbert* or $10/4\pi = 0.79577$ *amp-turn.*

$$\mathfrak{F} = 4\pi N I_{\text{abamp}} \qquad \textit{gilberts}$$

with N = number of turns and I = *abamperes* in each.

Unrat.	Rat.
Unrat.: M.K.S. unit is the $1/4\pi = 0.079577$ *ampere-turn*	Rat.: M.K.S. unit is the *ampere-turn*
$\mathfrak{F}_1 = 4\pi N I_{\text{amp}}$	$\mathfrak{F}_{1r} = N I_{\text{amp}}$ *amp-turns*
where $\mathfrak{F}_1$ is the *mmf* and I_{amp} the current in *amperes.*	where $\mathfrak{F}_{1r}$ is the mmf in *ampere-turns* and I_{amp} the current in *amperes.*

(*b*) *Permeability* μ Equid.: Form: $(L^{-1}T)$ *sec/cm* or $1/v$. Space permeability μ_0, Relative Permeability μ/μ_0, a number.

C.G.S. mag. unit $\frac{\textit{gauss}}{\textit{oersted}}$, μ_0 taken as numerically unity.

[1] Bibliography 29.

Unrat.: M.K.S.U. of μ,

$$\mu_{01} = 1\,\frac{henry}{m}$$

$$= 10^7 \text{ C.G.S.U.}$$

numerical value of $\mu_{01} = 10^{-7}$ in system.

Rat.: M.K.S.U. of μ,

$$\mu_{01r} = \frac{1}{4\pi} \times \frac{henry}{m}$$

$$= \frac{10^7}{4\pi} \text{ C.G.S.U.}$$

numerical value of $\mu_{01r} = 4\pi \times 10^{-7}$ in system.

(*c*) *Permeance* $\mathcal{P} = \frac{S}{L}\mu$. Space Permeance $\mathcal{P}_0 = \frac{S}{L}\mu_0$.

Equid. form. *T secs*. C.G.S.M.U. $\frac{maxwell}{gilbert}$.

$$\mathcal{P} = \frac{S_{cm}{}^2}{L_{cm}}\mu,$$

and

$$\mathcal{P}_0 = \frac{S_{cm}{}^2}{L_{cm}}\mu_0,$$

where μ_0 is taken as unity.

Unrat.:

M.K.S.U. $10^9\,\frac{maxwells}{gilbert}$

$$= 10^9 \; secs = henry.$$

$$\mathcal{P}_1 = \frac{S_{m2}}{L_m} \times 10^{-7} \; henrys$$

or *ohm-sec*.

S_{m2} in m^2,
L_m in *meters*.

Rat.:

M.K.S.U. $\frac{10^9}{4\pi}\frac{maxwells}{gilbert}$

$$= \frac{10^9}{4\pi} \; secs = \frac{1}{4\pi} \; henry.$$

$$\mathcal{P}_{1r} = \frac{S_{m2}}{L_m} \times (4\pi \times 10^{-7})$$

henrys or *ohm-sec*

where $(4\pi \times 10^{-7})$ is the numerical value assigned to μ_{01r}.

(*d*) *Magnetic Flux* Φ. Equid. Form: $L^1M^{1/2}T^{-1/2}$ or $\sqrt{TW}$. C.G.S.M.U. *maxwell*.

$$\Phi = \mathfrak{F}\mathcal{P} \quad maxwells.$$

Unrat.: M.K.S.U. *pramaxwell* or *volt-sec* = 10^8 *maxwells*.

$$\Phi_1 = \mathfrak{F}_1 \mathcal{P}_1 \quad \textit{volt-sec}$$

$$= 4\pi N I_{amp} \cdot \frac{S_{m2}}{L_m} \times 10^{-7}.$$

Rat.: M.K.S.U. *pramaxwell* or *volt-sec* = 10^8 *maxwells*.

$$\Phi_{1r} = \Phi_1 = \mathfrak{F}_{1r} \mathcal{P}_{1r} \quad \textit{volt-sec}$$

$$= N I_{amp} \cdot \frac{S_{m2}}{L_m} \cdot 4\pi \cdot 10^{-7}.$$

(*e*) *Magnetic Circuit Energy W*. Equid. Form: L^2MT^{-2}. (*a*) When the flux Φ is extraneous or derived from a source other than that of the operating mmf $\mathfrak{F}_1$, as in the ordinary direct-current dynamo. C.G.S.M.U. the *erg*.

$$W = \frac{\mathfrak{F}\Phi}{4\pi} \quad \textit{ergs}.$$

Unrat.:

M.K.S.U. the *joule* = 10^7 *ergs*.

$$W_1 = \frac{\mathfrak{F}_1 \Phi_1}{4\pi} \quad \textit{joules}$$

$$= \frac{4\pi N I_{amp} \Phi_1}{4\pi} = N I_{amp} \Phi_1$$

joules.

$\mathfrak{F}_1$ in *amp-turns*/4π,

Φ_1 in *pramaxwells*.

Rat.:

M.K.S.U. the *joule*.

$$W_{1r} = W_1 = \mathfrak{F}_{1r} \cdot \Phi_{1r} \quad \textit{joules}$$

$$= N I_{amp} \Phi_{1r}.$$

joules.

$\mathfrak{F}_{1r}$ in *ampere-turns*,

Φ_{1r} in *pramaxwells*.

(*b*) When the flux Φ is intrant, or derived from the operating mmf as in the ordinary case of a coil excited by a storage battery, C.G.S. Formula

$$W = \frac{\mathfrak{F}\Phi}{8\pi} \quad \textit{ergs}.$$

Unrat.: M.K.S. formula.	Rat.: M.K.S. formula.
$W_1 = \frac{\mathfrak{F}_1\Phi_1}{8\pi}$ *joules,*	$W_{1r} = W_1 = \frac{\mathfrak{F}_{1r}\cdot\Phi_{1r}}{2}$ *joules.*
$= \frac{4\pi NI_{amp}\,\Phi_1}{8\pi}$ *joules.*	$= \frac{NI_{amp}\,\Phi_{1r}}{2}$ *joules.*

(*f*) *Reluctivity* $\nu = 1/\mu$. Equid. form. L^1T^{-1} or v.
Space Reluctivity $\nu_0 = 1/\mu_0$.
C.G.S.M.U. ν_0 taken numerically as unity.

Unrat.:	Rat.
M.K.S. unit 10^{-7} C.G.S.M.U.	M.K.S.U. $4\pi \times 10^{-7}$ C.G.S.M.U.
$\nu_{01} = 10^{-7}$ *m/sec*	$\nu_{01r} = 4\pi \times 10^{-7}$ *m/sec*
$= 1$ *yrneh-m*	$= 4\pi$ *yrneh-m*
value of ν_{01} in system 10^7.	value of ν_{01r} in system $10^7/4\pi$.

(*g*) *Reluctance* $\mathcal{R} = \frac{L}{S}\nu$. Space Reluctance $\mathcal{R}_0 = \frac{L}{S}\nu_0$.

Equid. Form: $(0, 0, -1) = T^{-1}$.

C.G.S.M.U. $\frac{gilbert}{maxwell}$.

$$\mathcal{R} = \frac{L_{cm}}{S_{cm2}}\cdot\nu, \qquad \mathcal{R}_0 = \frac{L_{cm}}{S_{cm2}}\cdot\nu_0,$$

where ν_0 is taken as unity.

Unrat.:	Rat.:
M.K.S.U. $10^{-9}\frac{gilberts}{maxwells}$ $= 1$ *yrneh.*	M.K.S.U. $= 4\pi\frac{gilberts}{maxwells}$.
$\mathcal{R}_1 = \frac{L_m}{S_{m2}} \times 10^7$ *yrnehs*	$\mathcal{R}_{1r} = \frac{L_m}{S_{m2}} \times \left(\frac{10^7}{4\pi}\right)$ *yrnehs*
L_m in meters S_{m2} in sq. m.,	or *mhos/sec.,*

where 10^7 is the numerical value of ν_{01}.	where $\left(\frac{10^7}{4\pi}\right)$ is the numerical value of ν_{01r}.

(*h*) *Inductance* $L = \frac{N\Phi}{I}$. Equid. form. (0, 0, 1) = T,

C.G.S.M.U. $\frac{\textit{maxwell-turns}}{\textit{abamperes}}$ or *abhenrys*

Unrat.:	Rat.:
M.K.S.U. = 10^9 C.G.S.U.	M.K.S.U. = 10^9 C.G.S.U.
= 10^9 *abhenrys*	= 10^9 *abhenrys*.
= 1 *henry*.	= 1 *henry*.

(*i*) *Flux Density* $B = \Phi/S$. Equid. Form: $(-1, 1/2, -1/2)$ or $\sqrt{TW}/S$.

C.G.S.M.U. *gauss* or $\frac{\textit{maxwells}}{\textit{sq. cm.}}$.

Unrat.:	Rat.:
M.K.S.U. = 10^4 *gauss*	M.K.S.U. = 10^4 *gauss*
$= 1 \frac{\textit{pramaxwell}}{\textit{sq. m.}}$.	$= 1 \frac{\textit{pramaxwell}}{\textit{sq. m.}}$.

(*k*) *Magnetising Force* or *Gradient of Magnetic Potential*, $H = \mathfrak{F}/L$.

Equid Form: $(0, 1/2, -3/2)$ or $\sqrt{P}/L$.

C.G.S.M.U. *oersted* or *gilbert* per *cm*.

Unrat.:	Rat.:
M.K.S.U. = 10^{-3} *oersted*.	M.K.S.U. = $4\pi \times 10^{-3}$ *oersted*
$1/4\pi$ amp-turn per meter.	= 1 *amp-turn per meter*.
$H_1 = \frac{\mathfrak{F}_1}{L_m}$	$H_{1r} = \frac{\mathfrak{F}_{1r}}{L_m}$
$= \frac{4\pi N I_{amp}}{L_m}$.	$= \frac{N I_{amp}}{L_m}$.

(*l*) *Magnetic Pole Strength* m. Equid Form: (1, 1/2, −1/2) or $\sqrt{TW}$. C.G.S.M.U. unnamed.

$$m = \frac{\Phi}{4\pi},$$

where Φ is the flux in *maxwells* emerging from a pole of strength m.

Unrat.:	Rat.:
M.K.S.U. the $\frac{\textit{pramaxwell}}{4\pi} = 10^8$ C.G.S.U.	M.K.S.U. the *pramaxwell*, $= 10^8/4\pi$ C.G.S.U.
$m_1 = \frac{\Phi_1}{4\pi}$,	$m_{1r} = \Phi_{1r} = \Phi_1$ *pramaxwells*,
where Φ_1 is the emergent flux in *pramaxwells* or *volt-secs*.	where Φ_{1r} is the emergent flux in *pramaxwells* or *volt-secs*.

(*m*) *Magnetic Moment* $\mathfrak{M} = mL$. Equid. Form: (2, 1/2, −1/2). C.G.S.M.U. unnamed.

$$\mathfrak{M} = mL_{cm}.$$

Unrat.:	Rat.:
M.K.S.U. = 10^{10} C.G.S.M.U.	M.K.S.U. = $10^{10}/4\pi$ C.G.S.M.U. = *pramaxwellmeter*.
$\mathfrak{M}_1 = m_1 L_m = \frac{\Phi_1 L_m}{4\pi}$,	$\mathfrak{M}_{1r} = m_{1r} L_m = \Phi_{1r} L_m = \Phi_1 L_m$,
where Φ_1 is the emergent flux in pramaxwells and L_m the distance between pole centers in meters.	where $\Phi_{1r} = \Phi_1$ is the emergent flux and L_m distance in meters.

(*n*) *Intensity of Magnetisation* $\mathfrak{J} = \mathfrak{M}/V = m/S$. Equid. Form: (−1, 1/2, −1/2) or $\sqrt{TW}/S$. C.G.S.M.U. unnamed.

$$\mathfrak{J} = \frac{\mathfrak{M}}{V_{cm3}} = \frac{m}{S_{cm2}}.$$

Unrat.:

M.K.S.U. = 10^4 C.G.S.M.U.

$$I_r = \frac{\mathfrak{M}_1}{V_{m3}} = \frac{m_1}{S_{m2}}.$$

Rat.:

M.K.S.U. = $10^4/4\pi$ C.G.S.M.U.

$$= 1\ \frac{\textit{pramaxwell}}{\textit{sq. meter}},$$

$$I_{1r} = \frac{\mathfrak{M}_{1r}}{V_{m3}} = \frac{m_{1r}}{S_{m2}} = \frac{\Phi_{1r}}{S_{m2}}.$$

(*o*) *Volume Energy of Flux Density in Medium of Permeability* μ.

Equid. Form: $(-1, 1, -2)$ or W/V.

C.G.S.M.U. *erg per cm*^{3}

$$w = \frac{W}{V} = \frac{HB}{8\pi} = \frac{B^2}{8\pi\mu}\ \frac{ergs}{cm^3}.$$

Unrat.: M.K.S.U. the *joule per m*^{3} or 10 *ergs* per *cm*^{3}

$$w_1 = \frac{W_1}{V_{m3}} = \frac{H_1B_1}{8\pi}$$
$$= \frac{B_1^2}{8\pi\mu}\ \frac{joules}{m^3}.$$

Rat.: M.K.S.U. the *joule per m*^{3} or 10 *ergs* per *cm*^{3}

$$w_{1r} = \frac{W_{1r}}{V_{m3}} = \frac{H_{1r}B_{1r}}{2}$$
$$= \frac{B_{1r}^2}{2\mu_{1r}}\ \frac{joules}{m^3}.$$

(*p*) *Volume Energy Dissipated Hysteretically in Cycles of H and B.*

Equid. Form: $(1, 1, -2)$.

C.G.S.M.U. *erg per cm*^{3} *per cycle*

$$w = \frac{1}{4\pi}\int H \cdot dB = \frac{1}{4\pi}\int B \cdot dH\ \ \frac{ergs}{cm^3},$$

where the area of the cyclic loop is measured in œrsted-gausses and the volume energy dissipated is the 4πth part of this area.

Unrat.: M.K.S.U. *joule per m^3 per cycle*

$$w_1 = \frac{1}{4\pi}\int H_1 \cdot dB_1$$
$$= \frac{1}{4\pi}\int B_1 \cdot dH_1 \quad \frac{joules}{m^3}.$$

Rat.: M.K.S.U. *joule per m^3 per cycle*

$$w_{1r} = \int H_{1r} \cdot dB_{1r}$$
$$= \int B_{1r} \cdot dH_{1r} \quad \frac{joules}{m^3}.$$

(*q*) *Magneto-Mechanical Force between Equal Like Poles, F.*

Equid. Form: (1, 1, −2) or W/L.

C.G.S.M.U. the *dyne* or *erg per cm*

$$F = \frac{m^2}{\mu r^2_{cm}} \quad dynes,$$

where μ is the permeability of the medium and r is the distance between centers in *cm*.

Unrat.: M.K.S.U. the *joule per meter* = 10^5 *dynes*

$$F = \frac{m_1^2}{\mu_1^2 r_m^2} \quad \frac{joules}{meter}.$$
$$= \frac{\Phi_1^2}{(4\pi)^2 \times 10^{-7} r_m^2}$$

where $\mu_1 = \mu_{01} = 10^{-7}$ for free space.

Rat. M.K.S.U. the *joule per meter* = 10^5 *dynes.*

$$F = \frac{m_{1r}^2}{4\pi\mu_{1r} r_m^2}$$
$$= \frac{\Phi_{1r}^2}{(4\pi)^2 \times 10^{-7} r_m^2} = \frac{m_{1r} B_{1r}}{\mu_{1r}}$$
$$= \frac{m_{1r} B_{1r}}{4\pi \times 10^{-7}} \quad \frac{joules}{meter},$$

where $\mu_{1r} = \mu_{01r} = 4\pi \times 10^{-7}$ for free space.

(*r*) *Tractive Intensity or Tension per Unit Sectional Area, f.*

Equid. Form: (−1, 1, −2) or W/V.

C.G.S.M.U. *dyne per cm^2*

$$f = \frac{1}{4\pi} \cdot \frac{B^2}{2\mu} \quad \frac{dynes}{cm^2},$$

where B is the uniform flux density in gausses along the direction of which the tension is exerted, and μ is the space permeability = 1 in free space.

Unrat.:

M.K.S.U., the $\dfrac{\textit{joule per m}}{m^2}$

$$f_1 = \frac{1}{4\pi}\frac{B_1^2}{2\mu_1} \quad \frac{\textit{dyne-fives}}{\textit{square meter}},$$

where B_1 is in *pramaxwells per* m^2 and $\mu_1 = 10^{-7}$ in free space.

Rat.:

M.K.S.U., the $\dfrac{\textit{joule per m}}{m^2}$

$$f_{1r} = \frac{B_{1r}^2}{2\mu_{1r}} = \frac{H_{1r}B_{1r}}{2} \quad \frac{\textit{dyne-fives}}{\textit{square meter}},$$

where B_{1r} is in *pramaxwells per* m^2 and $\mu_{1r} = 4\pi \times 10^{-7}$ in free space.

(*s*) Mechanical Force F exerted on a Hypothetical Magnet Pole m in a Region of Gradient of Magnetic Potential, H.

Equid. Form: (1, 1, − 2)

C.G.S.M.U. the *dyne*.

As above indicated, the work W ergs done by the displacement of an extraneous flux Φ *maxwells* with respect to a mmf or potential difference $\mathfrak{F}$ *gilberts* is

$$W = \frac{\mathfrak{F}\Phi}{4\pi} \quad \textit{ergs}.$$

A pole m has a theoretical total emergent flux of $4\pi m$ *maxwells*. Or

$$W = \frac{\mathfrak{F}\cdot 4\pi m}{4\pi} = \mathfrak{F}m \quad \textit{ergs}.$$

Hence,

$$F = \frac{dW}{dl_{em}} = m\cdot\frac{d\mathfrak{F}}{dl_{em}} \quad \textit{dynes},$$

$$= mH \quad \textit{dynes}.$$

Unrat.: M.K.S.U. the *joule per meter* or *dyne-five*.

$$W_1 = \frac{\mathfrak{F}_1\Phi_1}{4\pi} \quad \textit{dyne-fives}$$

Rat.: M.K.S.U. the *joule per meter* or *dyne-five*.

$$W_{1r} = \mathfrak{F}_{1r}\cdot\Phi_{1r} = \mathfrak{F}_{1r}\Phi_1$$

$= \mathfrak{F}_1 m_1$ *dyne-fives*

or

$$F_1 = m_1 \frac{d\mathfrak{F}}{dl_m} = m_1 H_1,$$

where H_1 is in $\frac{amp\text{-}turns}{4\pi\ meter}$,

or

$$F_1 = \frac{m_1 B_1}{\mu_1} \quad dyne\text{-}fives,$$

where B_1 is the flux density or strength of magnetic field in *pramaxwells per sq. m.*, and μ_1 is the permeability of the region. In free space, $\mu_{01} = 10^{-7}$.

$= \mathfrak{F}_{1r} m_{1r}$

$$F_{1r} = F_1 = m_{1r} \frac{d\mathfrak{F}}{dl_m} = m_{1r} H_{1r},$$

where H_{1r} is in $\frac{amp\text{-}turns}{meter}$,

or

$$F_{1r} = F_1 = \frac{m_{1r} B_{1r}}{\mu_{1r}} \quad dyne\text{-}fives.$$

μ_{1r} is the permeability of the region. In free space $\mu_{01r} = 4\pi \times 10^{-7}$.

(*t*) Mechanical force F exerted, and work W done upon a hypothetical single magnet pole of strength m, carried once completely around a wire carrying a continuous current I.

C.G.S.M.U. From (*s*),

$$W = \frac{\mathfrak{F}\Phi}{4\pi} = \mathfrak{F} m \quad ergs,$$

where $\mathfrak{F} = 4\pi I$ *gilberts* is the cyclic mmf around the wire, m is the pole strength, $\Phi = 4\pi m$ maxwells, the theoretically emergent flux.

$$F = \frac{W}{L_{cm}} = \frac{W}{2\pi r_{cm}} = \frac{2Im}{r_{cm}} \quad dynes,$$

where r_{cm} is the radius in cm of a circular path concentric with the wire. I current in *abamperes*.

Unrat.: M.K.S.U.

$$W_1 = \frac{\mathfrak{F}_1 \Phi_1}{4\pi} = \mathfrak{F}_1 m_1 = I_{amp} \Phi_1 \quad joules,$$

Rat.: M.K.S.U.

$$W_{1r} = \mathfrak{F}_{1r} \Phi_{1r} = \mathfrak{F}_{1r} \Phi_r = I_{amp} \Phi_{er} = \mathfrak{F}_{1r} m_{1r} \quad joules,$$

where $\mathfrak{F}_1 = 4\pi I_{amp}$,	where $\mathfrak{F}_{1r} = I_{amp}$ *ampere-turns*,
$\Phi_1 = 4\pi m$ *pramaxwells* or *volt-sec*,	$\Phi_{1r} = \Phi_1 = m_{1r}$ *pramaxwells*,
$F_1 = \dfrac{W_1}{L_m} = \dfrac{W_1}{2\pi r_m}$	$F_{1r} = \dfrac{W_{1r}}{L_m} = \dfrac{W_1}{2\pi r_m}$
$= \dfrac{2I_{amp}m_1}{r_m}$ *dyne-fives* or *joules/m*.	$= \dfrac{I_{amp}m_{1r}}{2\pi r_m} = \dfrac{I_{amp}\Phi_{1r}}{2\pi r_m}$ *dyne-fives* or *joules/m*.

Conclusions.—The foregoing formulas indicate that in the process of rationalisation of magnetic circuit units, the magnitudes of flux Φ, flux-density B, and inductance L, remain unchanged. The same is evidently true of magnetic energies W, tractive forces F, and volume energies w.

The rationalised unit magnitudes of $\mathfrak{F}$, H, ν_0 and $\mathcal{R}$ are 4π times greater than the corresponding unrationalised unit magnitudes. On the other hand, the rationalised unit magnitudes of μ_0, $\mathcal{P}$, m, $\mathfrak{M}$ and $\mathcal{I}$ are 4π times smaller than the corresponding unrationalised unit magnitudes.

Rationalising the units of the practical magnetic system, involves, as above indicated, a marked simplification in the formulas for $\mathfrak{F}_{1r}$, Φ_{1r}, W_{1r}, H_{1r}, m_{1r}, $\mathfrak{M}_{1r}$, w_{1r}, w_{h1r}, and f_{1r}, by the elimination of the illogical 4π constant.

The logical consequence of magnetic-unit rationalisation, is rationalisation also in the units of the practical electrostatic circuit. In the rationalised M.K.S. system, the value of space permeability μ_{01r} is, as already indicated, $4\pi \times 10^{-7}$. Similarly, the value in that system of space permittivity $k_{01r} = \dfrac{10^7}{4\pi v^2} = 8.85 \times 10^{-12}$; so that the product $k_{01r}\mu_{01r} = 1/v^2$, where $v = 2.998 \times 10^8$ *m*/sec, the electromagnetic propagation velocity. Under this convention, a number of simplifications occur. Thus, electric displacement, and electric flux, become numerically equal to the electric quantity Q in coulombs, or the charge producing the flux; whereas in the C.G.S. system the flux is 4π times the generating quantity.

Such papers and books as have already been published in the M.K.S. (Giorgi) and C.G.–S.S. (Dellinger-Bennett) systems [1] have employed rationalised units.

Those who oppose the rationalisation of any practical system (Q.E.S., M.K.S. or G.G.–S.S.), point out that with the C.G.S. system unrationalised, there would be a divergence between the fundamental and practical systems, in many pairs of corresponding working formulas. Those who favor rationalisation, claim that the simplification in the working formulas thereby introduced, more than offsets this loss of parallelism.

The question of rationalisation is one which must be decided internationally and should not be decided hastily. It may be desirable to allow sufficient time for the crystallisation of opinion among the various countries.

BIBLIOGRAPHY

1. Maxwell, J. Clerk. "A Treatise on Electricity and Magnetism," 1881.
2. Rücker, A. W. "On the Suppressed Dimensions of Physical Units," *Phil. Mag.*, Feb. 1889, Vol. 27, pp. 104–114.
3. Heaviside, O. "Collected Papers," Vol. 2, 1892; also "Electromagnetic Theory," Electrician Series, London, Vol. I, 1893, Chap. II, pp. 116–125.
4. "Report of Committee on Units and Standards," *Trans. A.I.E.E.*, Oct. 1891, Vol. 8, pp. 532–543.
5. "Proceedings of International Electrical Congress of Chicago," Chamber of Delegates, 1893.

5a. Blondel, A. "Sur la question des unités magnétiques," As. Fr. pr. l'av. des Sciences Carthage, 1896, pp. 1-13.

6. Text of Report, Committee on Units at Paris, *Trans. A.I.E.E.*, 1900, Vol. 17, p. 552.
7. "Congrès International d'Electricité," Proces Verbaux, Paris, 1901.
8. Giorgi, G. "Unità Razionali di Elettromagnetisms," Atti del' A.E.I., 1901, p. 402.
9. Ascoli, M. "On the Systems of Electrical Units," *Trans. Int. El. Cong. of St. Louis*, 1904, Vol. 1, pp. 130–135.
10. Emde, F. "Treatment of Possible Systems," E.T.Z., 1904.

10a. Fleming, J. A. "Units, Physical," *Encycl. Brit.*, 11th Edition, Vol. 27, pp. 738–744, 1911.

11. Karapetoff, V. "The Magnetic Circuit," 1911.
12. Dellinger J. H. "International System of Electric and Magnetic Units," Sc. Paper of Bureau of Standards No. 292, 1916; also "Rationalisation of the Magnetic Units," *Elec. World*, N. Y., Vol. 62, pp. 810-812.
13. Bennett, E. "A Digest of the Relations between the Electrical Units and the Laws underlying the Units," *Univ. of Wisconsin Bulletin*, Nov. 1917, 93 pp.
14. Hazeltine, L. A. "Electrical Engineering," *The Macmillan Co.*, N. Y., 1924, 625 pp.
15. Bennett, E. and Crothers H. M. "Introductory Electrodynamics for Electrical Engineers," *McGraw-Hill Book Co.*, 1926, 665 pp.; also "Experiments in Electrodynamics," *Univ. of Wisconsin*, 1927.

[1] Bibliography 8, 12, 13, 14, 15.

16. Kennelly, A. E. Historical Outline of the Electrical Units," *Proc. Soc. Prom. Engrg. Education*, July 1928, Vol. 19, pp. 229–275, with additional bibliography.
17. Kennelly, A. E. "Magnetic Circuit Units," *Trans. A.I.E.E.*, Apr. 1930, Vol. 49, No. 2, pp. 486–510, additional bibliography, *Rev. gén. de l'Elect.*, June 14, 1930.
18. Janet, P. "A propos de la question des unités magnétiques," *Rev. gén. de l'Elect.*, 14 June 1930.
19. Kennelly, A. E. "Units and Definitions adopted by the 1930 Plenary Assembly of the I.E.C. in Scandinavia," *Electrical Engineering N. Y.*, Feb. 1931, pp. 141–142. *Revue générale de l'Electricité*, Nov. 8, 1930, Vol. 28, pp. 719–722.
20. Brylinski, E. "Observations sur les décisions de la Commission électrotechnique internationale concernant les unités magnétiques," *Rev. gén. de l'Elect.*, 8 Nov. 1930, Vol. 28, pp. 722–725.
20a. G. W. O. Howe. "Magnetic Units," *The Electrician*, Nov. 28, 1930, pp. 683–684.
21. Blondel, A. "Sur les unités magnétiques, Un plaidoyer pour la logique," *Rev. gén. de l'Elect.*, Oct. 11, 1930, Vol. 28, pp. 555–560; also "Le Henry et les autres unités magnétiques pratiques," *Rev. gén. de l'Elect.*, 27 Dec. 1930, Vol. 28, pp. 1033–1037.
22. Brylinski, E. *Comptes Rendus*, 17 Nov. 1930, *Rev. gén. de l'Elect.*, 27 Dec. 1930, p. 1037.
23. Mailloux, C. O. "La Question du 'Gauss.' La question de la responsibilité: Les diverses définitions de l'expression 'flux magnétique,'" *Rev. gén. de l'Elect.*, Jan. 24, 1931, Vol. 29, pp. 125–135.
24. Kennelly, A. E. "Magnetic-Circuit Units Adopted by the I.E.C.," *Trans. A.I.E.E.*, N. Y., Jan. 1931, pp. 1–5.
25. Blondel, A. "Perfectionnement du système actuel d'unités électromagnétiques," *Comptes Rendus*, Vol. 192, p. 124, 19th Jan. 1931, and "Rationalisation des équations électromagnétiques," *Comptes Rendus*, Vol. 192, p. 318, 9th Feby. 1931.
26. Brylinski, E. "Sur les systèmes d'unités," *Rev. gén. de l'Elect.*, Feb. 7, 1931, Vol. 29, pp. 209–211.
27. Blondel, A. "Rationalisation des équations electromagnétique," Note à *l'-Academie des Sciences*, 9 Feby. 1931.
28. Kennelly, A. E. "The Œrsted Considered as a New International Magnetic Unit," *The Scientific Monthly*, N. Y., Apr. 1931, pp. 379–380.
29. Kennelly, A. E. "The Convention of Equidimensional Electromagnetic Units," *Proc. Nat. Ac. Sc.*, Washington, Vol. 17, Mar. 1931, pp. 147–153.
30. Budeanu, C. I. "La question des grandeurs et unités électriques et magnétiques," *Pub. com. Elect. Roumain* No. 11. Feby. 1931, Bucarest.

THE CHANGES WHICH GASEOUS IONS UNDERGO WITH TIME

By JOHN ZELENY

(*Read March 6, 1931*)

When a molecule of a gas becomes ionized the process as a rule consists in the removal of an electron from the neutral molecule,[1] so that initially the negative ion has the dimensions of an electron and the positive ion those of a molecule. Do these initial states of the two ions persist or do the ions undergo change by the attachment to them of neutral molecules? If molecules do become attached to the ions are they the main constituent molecules of the gas or are they molecules of some impurity? If changes do occur how long a time is involved between the ionization process and the time when the ion assumes its final form? These questions have been the subject of much study but no final answer for them has as yet been found.

It is definitely known that electrons remain free in some gases for considerable periods of time whereas in other gases they become attached in an immeasurably short time to one or more neutral molecules to form negative ions, which differ in size somewhat but not greatly from the positive ions in the gas.

The force of attraction between an ion and a neutral molecule is much larger than that between two neutral molecules and this force may bring about the formation of a molecular cluster around an ion even when such a cluster of neutral molecules in the particular gas could form only on extremely rare occasions if at all.

The molecules of a gas in the neighborhood of an ion become polarized by the displacement of their two kinds of electricity relative to each other, and as a consequence a force

[1] H. D. Smyth, *Roy. Soc. Proc.*, A104, 121, 1923.

of attraction between the ion and the molecules is brought into play. The attachment of the molecule to the ion is however made difficult by the large relative velocity at impact arising in part from this attractive force itself and in part from the motions of thermal agitation.

In the molecules of some gases the two electricities are normally distributed in an unsymmetrical manner and these are said to possess permanent dipoles owing to which they are attracted by an ion with a much larger force than are those molecules in which only temporary dipoles are produced by the action of the ion.

Another class of bodies may be formed in a gas by the ionizing agent itself upon which also the ions may exert an unusually large attractive force. These bodies are molecules in the so-called excited state in each of which an electron has been temporarily displaced farther from the nucleus than its normal position, giving to the molecule dipole properties.

Evidence [2] that attractive forces of the kind discussed may cause attachment between ions and neutral molecules is to be found in the results of much work done in recent years on mobilities of ions showing that ions under some circumstances do change with time.

Even in my early determinations [3] of the mobilities of ions formed in gases by x-rays, it was found that the values obtained diminished as the time taken for the ions to cross the space between two electrodes increased. The tentative explanation of this behavior adopted at the time was that the change observed was only an apparent one arising from the effects of diffusion. Since that time diffusion has often been arbitrarily invoked to explain certain spreading effects observed in mobility measurements.

In some recent work [4] on the mobilities of ions the effects of diffusion were carefully studied and for the first time evaluated so that correction for them could be made. The

[2] H. A. Erikson, *Phys. Rev.*, **20**, 117, 1922; **24**, 502 and 622, 1924.

[3] J. Zeleny, *Phil. Trans. Roy. Soc.*, **A195**, 193, 1900.

[4] J. Zeleny, *Phys. Rev.*, **34**, 310, 1929.

results of the measurements which were made on ions over 2 seconds after their formation in air containing about 2 mg. of water per liter showed that ions of this age do not all have the same mobility. No separate kinds of ions were resolved but the ions of each sign were found to consist of an apparently continuous group of mobilities in which the fastest ions had mobilities about 45 per cent greater than the slowest for positive ions and about 30 per cent greater for negative ions. Moreover while the numerical distribution of the ions on the two sides of the most abundant kind was a symmetrical one for positive ions this was not the case with the negative ions. Groups of ions of differing mobilities have also been observed by other investigators,[5] notably by Laporte. The existence of ions in groups in which the ions do not all have the same mobility can only mean that the ion structure is not simple and that in time the initial ions become surrounded more or less by neutral molecules.

In later experiments,[6] the air in which the ions were formed was thoroughly dried by passage through coils and filters immersed in liquid air, and the negative ions, about two seconds after their production, were found to consist of two groups. The most abundant ions of the faster group had a mobility of 2.45 cm./sec. whereas in undried air the "peak" mobility of the single group there observed was but 2.08 cm./sec. The slower and less numerous group of negative ions had a mobility of about 1.45 cm./sec. The positive ions in the dried air formed a fairly wide group having a "peak" mobility of 1.05 cm./sec. only, although in undried air the positive ions of the same age had the much larger "peak" mobility of 1.36 cm./sec.

These widely different values found for the mobilities of old ions in air have led me to extend the work to determinations of the mobilities of ions shortly after their formation and at determined times subsequently. The method employed in making the measurements is essentially the same as

[5] M. Laporte, *Ann. de Physique*, **8**, 466, 1927; J. L. Hamshere, *Roy. Soc. Proc.*, **127**, 298, 1930.

[6] J. Zeleny, *Phys. Rev.*, **35**, 35, 1930.

that used previously [4] and consists in having ions move under the influence of an electric field from the outer of two concentric cylinders toward the inner one and finding how far they are blown down stream during their passage by a steady stream of gas flowing axially in the space between the cylinders. The manner in which the ions are distributed along the inner cylinder, on landing there, is found by moving the whole cylinder axially by steps and measuring for each step the ion current flowing to a narrow insulated section of the cylinder.

The changes in the apparatus which made it possible to measure the mobility of the ions very soon after their formation are shown in Fig. 1, where a portion of the two con-

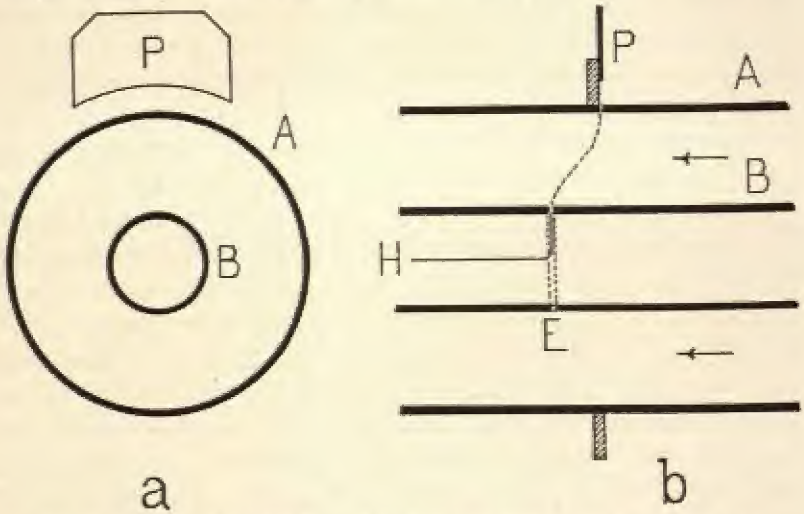


Fig. 1.—Portion of apparatus showing how ions were produced in gas stream.

centric cylinders is drawn in transverse section at "a" and in longitudinal section at "b". A and B represent the two cylinders between which the air stream flows. A nickel plate P, 1 mm. thick, having on its lower edge a strong deposit of polonium, for whose preparation I am indebted to Prof. A. F. Kovarik, is placed above the outside cylinder immediately over some narrow openings, 1 mm. wide, made

in the cylinder along a circumference. These openings in the cylinder were sealed air tight by two layers of thin aluminum foil placed over them on the inner surface of the cylinder. Absorption screens were also placed over the openings on the outside of the cylinder to limit the range of the alpha rays from the polonium to a short distance within the cylinder. The ions whose mobility was being measured were thus formed by the alpha rays within the gas stream itself and they were set into motion radially by the electric field between the two cylinders as soon as they were formed and rapidly removed from the products of any chemical action produced by the rays.[7] The dotted line in Fig. 1 indicates the paths of ions as they travel from the outer cylinder to the insulated collecting section E which is connected by the wire H to the electrometer used for measuring the ion current. During a measurement of their mobility the age of the ions ranges from zero to the time taken for them to cross the space between the two cylinders. This latter time can be changed by altering the voltage of the outer cylinder, the inner one being always joined to earth. Actually the potential of the outer cylinder was varied between 5 volts and 300 volts and this changed the time taken for the ions to cross from one cylinder to the other from about 0.68 sec. to 0.008 sec.

The lower time limit was imposed by the fact that with the most rapid gas stream used, which however was limited to a mean velocity of 10 cm./sec. to avoid danger of turbulent motion, the ions were carried down stream less than one millimeter when the potential of the outer cylinder was 300 volts, and distances much smaller than this could not be measured with any approach to accuracy. On the other hand when a low voltage is placed on the outer cylinder not only are more ions lost by recombination but the ions reaching the inner cylinder are distributed over a considerable distance along it so that the collecting ring at any place receives so few ions as to make accurate measurement difficult; and

[7] A. M. Tyndall, G. C. Grindley and P. A. Sheppard, *Roy. Soc. Proc.*, **A121**, 185, 1928.

besides this the peak of the distribution curve is less sharply defined and hence less easily located.

Some curves obtained with positive ions in dry air and giving typical distributions of the ions along the inner cylinder are shown in Fig. 2. The abscissas give the positions of the

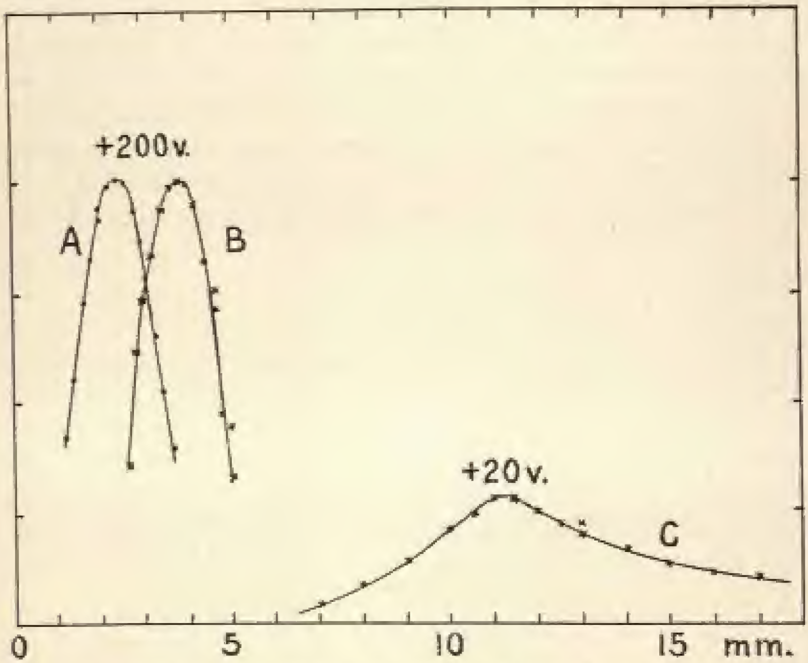


FIG. 2.—Ion currents as ordinates plotted against positions of collector, showing distribution of ions after landing on inner cylinder.
Curve A. + 200 volts. No air stream.
Curve B. + 200 " . Air stream, 9.7 cm./sec.
Curve C. + 20 " . Air stream, 6.2 cm./sec.

collecting ring (E, Fig. 1) and the ordinates the corresponding ion currents. Curve A gives the distribution for + 200 volts on the outer cylinder when the air stream was not flowing, and curve B shows the distribution with an air stream. Except for a displacement down stream, curve B is a duplicate of curve A, indicating that at this age (0.014 sec.) the ions are all of one kind.

Curve C was obtained with + 20 volts on the outer

cylinder and with a somewhat slower air stream than was used for curve B. Here the ions are spread over a much larger range than was the case in curve B, and the distribution about the peak is far from symmetrical. Only a part of this spreading arises from diffusion effects; the remainder is due to the presence of ions of different mobilities.[4]

In Fig. 3 are shown the "peak" mobilities obtained from

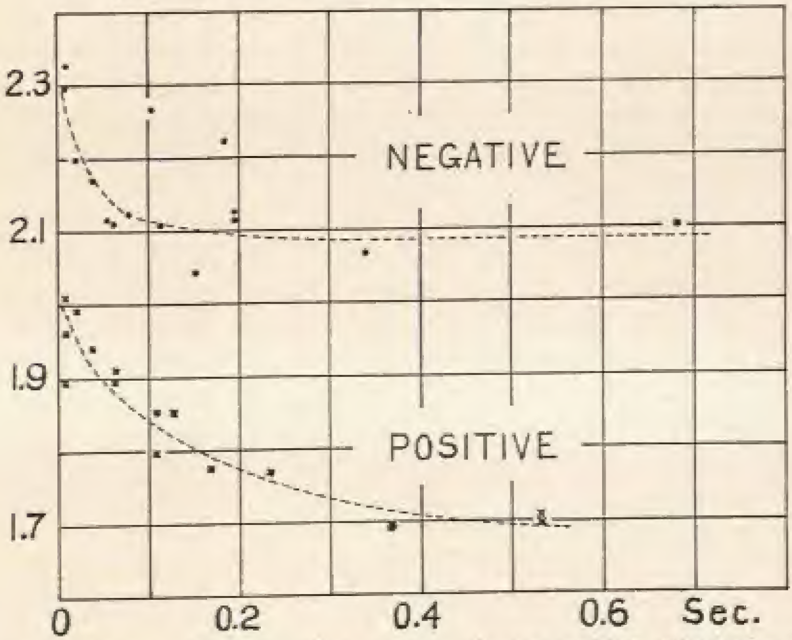


FIG. 3.—Variation of mobility with age of ions. Air dried by KOH. Mobilities are plotted as ordinates in cm./sec. per volt/cm.

distribution curves such as are given in Fig. 2 when the time taken for the ions to cross the condenser was varied. The air used in these measurements was at atmospheric pressure and only partially dried by passage through a large container filled with sticks of pure fused caustic potash. The times taken for the ions to cross between the two cylinders are given as abscissas and the corresponding mobilities are given as ordinates in cm./sec. per volt/cm. The values of the mobilities given in this and succeeding figures are subject to

corrections, which have not been applied, to reduce them to standard conditions of pressure and temperature. Such combined corrections are nearly always to be subtracted and may be as large as 2 per cent.

It is seen from the figure that the mobilities for both positive and negative ions decrease with age, the decrease for positive ions being about 15 per cent in 0.5 sec., and that for negative ions being considerably less. The deviation of some of the observed values from the median curve are too large to be accounted for by experimental errors alone, and indicate that some minor and undetermined change of conditions was able to produce a substantial alteration in the mobility of the ions.

Measurements were next made in air containing about 4 milligrams of water per liter. The values of the mobilities obtained on different days showed large divergences among themselves. However when the mobilities were determined for positive and negative ions, one after the other, with the same voltage on the outside cylinder the values obtained were essentially identical in magnitude so long as the age of the ions was less than about 0.04 sec., after which age the positive ions moved the slower. When the time taken for the ions to cross between the cylinders was 0.01 sec. the values of the mobilities in this undried air were approximately 2.0 cm./sec.

Inasmuch as the results obtained with this same apparatus previously[4, 6] were invariable when much older ions were used it would appear that while the final state of ions in the presence of some impurity may be fixed their rate of transformation during the early part of their life is dependent greatly as would be expected upon the amount of this impurity in the gas.

Finally, mobility measurements were made in thoroughly dried air. For this purpose the air was passed, first through large jars filled one with granulated calcium chloride and the other with broken sticks of pure caustic potash, and then through copper coils and a filter, both immersed in liquid air. The filter was filled partly with metal turnings and partly

with closely packed glass wool that stopped the passage of ice crystals which formed in the coils and which on becoming detached were blown along by the air stream. Notwithstanding the pre-drying of the air by the chemical means used, enough ice crystals were formed in the coils immersed in liquid air to clog the passage of the air after a use of about three hours. This necessitated the dismantling of a part of the apparatus to drive off the condensed moisture and as will be shown later this procedure had an influence on the values of the mobilities obtained immediately afterwards.

Figure 4 shows some of the results obtained when very

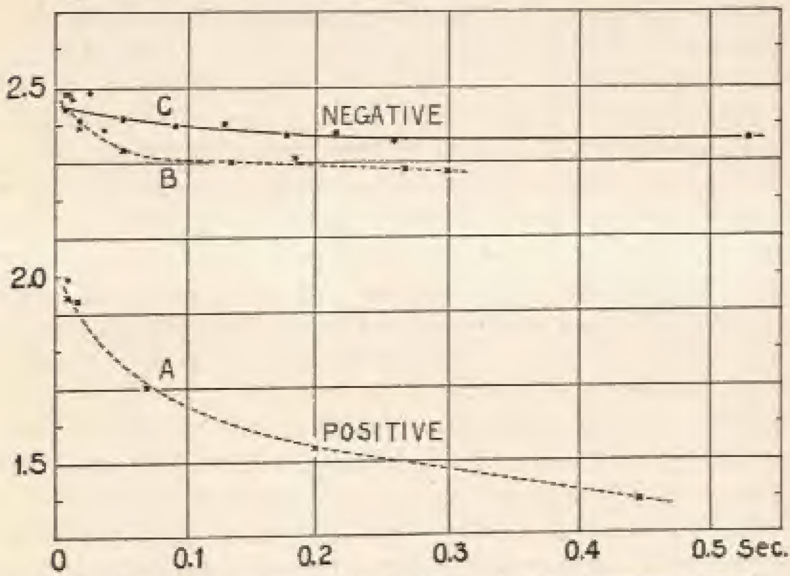


Fig. 4.—Variation of mobility with age of ions. Air dried by passage through coils and filter immersed in liquid air.
Curves A and B. Early observations.
Curve C. Negative ions after prolonged use of apparatus.

dry air was used and indicates the way in which the mobilities of the two kinds of ions were found to change with age. Here again the abscissas represent the times taken for the ions to complete their journey and the ordinates give the corresponding mobilities at the peaks of the distribution curves.

Curves A and B show the mobilities of the positive and

negative ions respectively for the first series of observations made with the highly dried air. The conditions appeared at first much more stable than was the case with the observations made previously. Curves A and B show the same characteristics as do the corresponding ones in Fig. 3 but the actual changes of the mobilities with age are different.

After dry air had been used continuously in the apparatus for some weeks the values obtained for the mobilities of positive ions more especially began to increase very considerably although not regularly. Suspecting that the copper coils and filter used for freezing the moisture out of the air might be the source of some contamination, these were replaced by similar coils and a filter made of pyrex glass. However, the values obtained for the mobilities after this change had been made were about the same as the highest ones previously found when copper cooling coils were used.

To make certain that steady conditions prevailed during a measurement, it was customary to repeat the observations for a distribution curve once or twice after the first set had been completed. These sets of observations always gave concordant results for negative ions and at first did so for positive ions as well, but after the stage had been reached where relatively high values were being obtained for the mobilities of the positive ions it was found that distribution curves for these ions taken in succession during the same run began to give peak mobilities of an increasing magnitude up to an apparent limit, and at times evidence was found of a second peak on the distribution curve indicating a group of ions of lower mobility than that corresponding to the main peak in the curve.

Unfortunately, as stated above, the cooling coils of the drying system had to be removed from the apparatus periodically in order to remove the ice crystals that had collected in them. During this process, while the coils were being thoroughly heated, a stream of dry air was used for removing the moisture. The coils were connected to the apparatus by overlapping the glass tubes to be joined and the junction was

first covered with a tight-fitting layer of tin foil and then wrapped with tape to make it gas tight. After the connections had been made and the coils cooled with liquid air, air was run through the whole apparatus system for about 20 minutes before readings were begun, and yet in every case the peak mobility of the positive ions returned to a lower value than it had had before the coils had been removed from the apparatus, and the mobility increased again to its former high value only after a more prolonged run of dry air through the apparatus. It thus appears that during this process of removal of the water from the cooling coils something is introduced that temporarily lowers the mobility of a part of the positive ions. Efforts to locate the cause have as yet met with no success.

The behavior which has just been described is illustrated

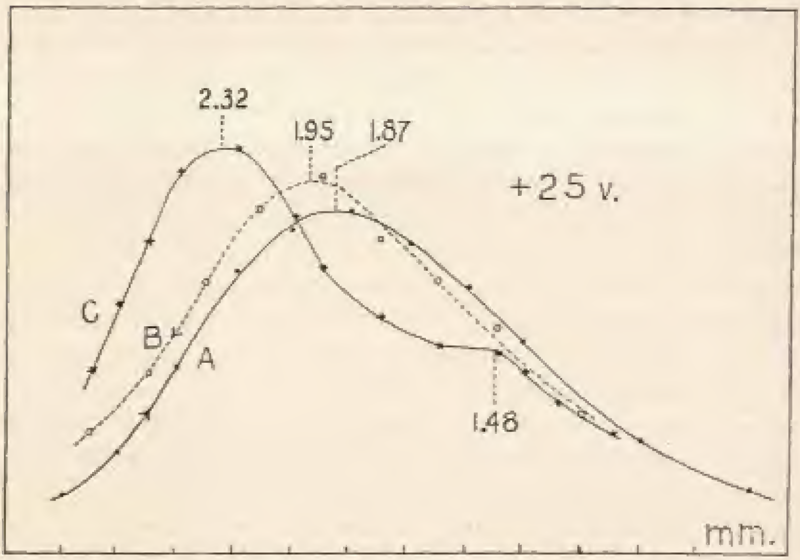


FIG. 5.—Change of distribution of positive ions during a continued series of observations taken in the order A, B, C. Numbers on curves give mobilities in cm. per sec. per volt per cm. for points indicated.

by the distribution curves shown in Fig. 5 where the ordinates represent the ion currents reaching the collecting ring for different positions of the latter as given by the abscissas.

The potential on the outside cylinder was + 25 volts and the different curves were taken in succession during one continuous run. Mobilities corresponding to the different peaks are indicated on the curves in terms of cm./sec. per volt/cm.

Curve A represents the readings taken first, the order being indicated by the arrow head. The time required for this set was about one hour. The readings were next repeated in the reverse order and these are given by curve B, which shows the peak moved in such a direction as to indicate an increase of mobility. A set of readings was next taken with a different voltage on the cylinder and then observations were again made with + 25 volts which on repetition showed no appreciable change. Further measurements were prevented by the air system becoming clogged by ice crystals.

Curve C represents the last set of readings and shows that the mobilities of a part of the ions had again increased in value while some of them retained the lower mobility given by the low peak at the right side of the curve. The ions of the left hand peak of curve C required one-ninth of a second to cross the air stream. The high value of 2.32 cm./sec. for the mobility of these positive ions was approximately repeated several times and similar values were obtained with + 50 volts; and these are the highest values as yet reported for such ions. These values are only slightly lower than those obtained for negative ions of like age.

A summary of the later results obtained for the mobilities of negative ions of different ages in this extremely dry air is given in curve C of Fig. 4, where the points for the shortest ages are averages of two or more values. These values decrease from 2.44 cm./sec. at an average age of 0.004 sec. to 2.35 cm./sec. at an average age of 0.26 sec. No summary of final values for positive ions is given because of the high susceptibility of these values to slight and unknown changes of conditions, which caused the results obtained to vary all the way between curve A of Fig. 4 and the high values mentioned in the preceding paragraph. The behavior of positive

ions here described, in having their mobilities highly sensitive to what appears to be small traces of impurities, is a confirmation of similar results reported by Tyndall and Powell,[8] whose paper was received while this work was in progress. They obtained a value as high as 2.1 cm./sec. for the mobility of positive ions in extremely pure nitrogen and as high as 17 cm./sec. in helium. Schilling[9] gives 1.9 cm./sec. as the highest value he obtained for positive ions in air when he used for his apparatus a quartz vessel which could be thoroughly baked out and exercised great care to eliminate impurities from the gas.

It is to be noted that the distribution curves do not show the two types of ions so characteristic of Erikson's results[2] and which Mahoney[10] did not observe in very dry air and which Schilling[9] found only when a small amount of moisture is present. The most interesting part of the life history of an ion may well be within the first thousandth of a second of its existence, and unless highly turbulent gas streams are used this region is beyond the possible limits of blast methods of measurement. In previous work[6] with older ions in highly dry air a slow type of ion became sufficiently prominent for definite resolution. This type was not observed here with ions of shorter age and it would seem therefore that its presence was due to collisions of very rare occurrence, or to some impurity now absent. The exceptionally low value (1.05 cm./sec.) previously found for the mobility of old positive ions in dry air, in the light of the present results, must be ascribed to the presence of some such impurity as at first affected the results here and which was later removed with great difficulty and even then perhaps only in part.

In moist air, although the mobilities of the positive and negative ions were here found to be identical at very short ages, nevertheless this value was approximately 20 per cent below that found for negative ions in dry air. This fact again points to a cluster form of ion.

[8] A. M. Tyndall and C. F. Powell, *Roy. Soc. Proc.*, **A129**, 162, 1930.

[9] H. Schilling, *Ann. der Phys.*, **83**, 23, 1927.

[10] J. J. Mahoney, *Phys. Rev.*, **33**, 217, 1929.

The changes in the mobility of ions with age which have been described in the previous pages can best be accounted for by supposing the ions in air to be molecular clusters whose structure is greatly dependent upon the presence of small traces of impurities. The apparently continuous change over a fairly long period of time in the mobility of the positive ions more especially as shown by the curves of Figs. 3 and 4, indicates that a considerable number of molecules is involved in the formation of each ion. Moreover this long time required for the ions to attain their final state shows that the molecules which do become attached to the ions must either be extremely rare or that the probability of their adherence at a collision is exceedingly small or that both of these conditions are satisfied. It may be that some of the molecules of the chief gas become attached to the ion first and that later these are added to or slowly replaced during favorable collisions by other kinds of molecules which are rare but owing to polar properties are more strongly attracted by the ions.

No clue has as yet been found as to the identity of the molecules that produce the marked changes in mobility that have been observed, and some of which at least were removed from my apparatus during the first period of its use, as evidenced by the great reduction in the aging of positive ions in highly dried air.

The objection may rightly be made to blowing methods of measuring mobilities that the gases must be used in such large quantities as to make a high degree of purification impossible and also that the apparatus has to be so made that modern methods of outgassing the walls cannot be applied. By using air alone I have attempted to avoid the first objection, since nature supplies us here with a highly uniform even if not simple product. As regards impurities given off from the walls of the apparatus a blowing method at least has the virtue that such impurities are blown out of the apparatus as fast as they are liberated. Unfortunately it appears that the presence even of a small trace of impurity

may be sufficient to affect the ion structure. Workers using a closed apparatus with a static gas report that even after extreme care in outgassing the walls of the vessel the mobilities of the ions decrease with time after fresh gas has been admitted, indicating the gradual accumulation of impurities released from the walls of the vessel. If high values of the mobilities are indicative of absence of impurities it must be said that with my present apparatus I have obtained [6] for the mobilities of the negative ions in air values as high as any previously reported and for positive ions the values here given are higher than any obtained as yet by any method. That still higher purity is demanded if we are to acquire from mobility measurements certain knowledge regarding the behavior of an ion when surrounded by molecules of the parent gas alone, is indicated by these results with air as well as by those obtained by Tyndall and Powell [8] for helium and nitrogen.

I am indebted to my assistant, Mr. W. P. Cunningham, for valuable aid with the measurements recorded in this communication.

THE RAILROADS VERSUS THE WEATHER

By ROBERT DeC. WARD

(Read by title April 25, 1931)

Introduction.—Among the many controls of weather and climate over the varied activities of man there are few more striking than those noted in the construction and operation of railroads. Tracks and trains are outdoors, exposed from day to day to all the vicissitudes of the weather; without protection against snow, or wind, or excessive heat, or biting cold. "It is not easy," as the late Professor M. W. Harrington once expressed it, "to protect large boxes on wheels, flying along at 20 miles an hour or more, against the weather."

The better a construction engineer or a railroad man knows his climate, the better equipped he is to build and to operate his road. The available climatic data for most parts of the world are already reasonably satisfactory, and among them there are many that are of essential importance in connection with railroading. Climatology is ready to supply the engineering profession with all available information concerning the weather and climate of any particular region or place, and is, furthermore, not only willing but anxious to provide any additional details, not already available, if only the exact needs of the engineer are made clear. In this matter we have not yet reached the stage of fullest and most effective coöperation between those who collect and summarize the numerical data and those who need these data in the practical work of railroading.

During three or four decades past the writer has been collecting examples of weather controls over the construction and operation of railroads. These examples come from many parts of the world, and from many different climates. They were found in engineering and especially in railroad journals; in geographical publications; in books of travel, and in other

scattered sources of information.[1] The real value of any such investigation as that suggested in this paper lies in bringing together the results of past experience. Profiting by this experience, time, and money, and doubtless also human lives, may be saved. It is this directly practical side of the subject that appeals to the writer, and seems to him to justify a far more detailed and comprehensive study of it than has ever been undertaken, or than there is opportunity to attempt on this occasion. Such an investigation should classify the weather controls according to the types of climate in which they occur, for each climatic province or region has its own peculiar meteorological handicaps. Thus, when new railroad construction is planned for any particular region, the experience gained in other similar climates may serve as a practical guide in the new undertakings.

Railroad engineers, whether constructing or operating, should know beforehand whether they will have to deal with deep snows, with frequent heavy rains, with severe cold, with floods, with excessive heat, with droughts, with dust, with ice storms; with marked seasonal variations in the critical weather elements, and so on through a long list. Most, if not all, of this information is readily accessible in the regular official meteorological summaries, and in the discussions which usually accompany the tabulated data. A student of the atmosphere who examines the cases of weather controls over railroading cannot but be impressed by the fact that many of the mistakes made by the engineers might have been avoided if a careful study of the meteorological data had been made in advance. It is, of course, true that many of the new territories where railroads were built a good many years ago were then meteorologically more or less unknown. On the other hand, previous experience in similar climates

[1] A number of the examples referred to below were included in a thesis prepared by a graduate student (R. M. Brown: "Climatic Factors in Railroad Construction and Operation," *Journ. Geogr.*, Vol. 2, April, 1903, pp. 178–190). A few additional cases, not previously included in the writer's catalogue, were found in a more recent paper (G. H. Burnham: "The Weather Element in Railroading," *Monthly Weather Review*, Vol. 49, Jan. 1922, pp. 1–8, with bibliography).

might have furnished valuable information as to the handicaps that would have to be met and guarded against in the new country. In spite of the rapid increase in airplane transportation, thousands of miles of new railroad construction will be undertaken in the coming years, for the railroads will remain the great carriers of the world's heavy land freight, and will never lose all their passenger traffic. In laying out and in constructing these railroads, our rapidly increasing knowledge of the world's weather and climates will furnish the engineering profession with more, and also with more precise, information. With this information, added to experience, the future will provide fewer and fewer mistakes made because of failure to foresee and to guard against hostile weather conditions.

It is from the standpoint of the climatologist, not that of the engineer, that this problem is here considered. The illustrations that follow are taken more or less at random from a fairly long and growing list. They are snapshots, not a complete picture.

1. *The Railroads' Struggle with Winter.* (*a*) *Snow.*—Railroads have reached their greatest development in the north temperate zone, with its wide range of climates, from extreme continental to modified marine; from deserts to mountains; from latitudes snow-covered and frozen during their long winters to genial southern lands which only occasionally suffer from an invasion of cold from the north. In these thickly-settled north temperate zone lands, the effects of weather conditions upon the construction and operation of railroads have been far-reaching and of great economic importance. Most of the following examples are taken from this zone, and from the United States, because of our natural interest in our own country, and also because the variety of our climatic types provides a considerable and representative range of weather handicaps within our own borders. Further, our transportation systems are highly developed; American ingenuity and skill have devised new methods of dealing with these handicaps, and many of our problems are more difficult than are those in Europe.

In the higher latitudes of our continental climates winter snows, of greater or less depth and of longer or shorter duration, are one of the most expensive meteorological handicaps for the railroads within the "snow belt," an expense from which the roads near, or south of, the limits of more or less regular deep winter snows are exempt. The budgets of the southern railroads do not list the item, "snow removal." A mild, "open" winter in latitudes where snowfall is an important factor in railroad operation means a saving of money, time and labor; results in increased net income, and may contribute not a little to increased dividends. The motive power usually employed in fighting snow is then earning money for the companies. Few people outside of those who are directly concerned realize how carefully planned out, months in advance, is the campaign of a railroad against winter snows, and the same is true of the electric street railway systems in snow-belt cities. Every possible detail is thought out and provided for.

The margin between rain and snow is very narrow. It is one of the most critical demarkation points in nature. Whether rain or snow falls makes a tremendous difference in the economics of man's everyday life. A great deal, probably most, of the snowfall of the United States as a whole comes with temperatures within not more than 2° or 3° of freezing. It is, therefore, what might almost be called fortuitous whether rain or snow falls, for just as we may and do have snow when the thermometer reads above 32° Fahrenheit, so we may and do have rain when the temperature is below the freezing point. A 24-hour rainfall of 2 inches is fairly heavy for many portions of the temperate zone, yet unless continued day after day it seldom makes trouble because the water runs off by itself. Snow, on the other hand, relatively seldom accumulates on the level at a rate of over 2 inches an hour. This, under average conditions of snow density, is the equivalent of only about 0.2 inch of water. It is of the greatest importance to man that condensation proceeds slowly at low temperatures. If anything like the quantity of water which falls in a

cloud-burst were to come, in a short time, in the form of snow, it would amount to several feet in an hour. What this would mean to the railroads, and in the general activities of people as a whole, is almost hopelessly unimaginable.

Observations concerning the snowfall of the United States are available in sufficient number and detail to provide adequate information for those who are concerned with surface transportation undertakings. Thus, the mean annual and mean monthly depths of snowfall throughout the years of record; the maximum and the minimum depths in any year or any month; the average and extreme dates of first and of last snowfall; the numbers of days with snowfall (0.01 inch or more, when melted); the maximum 24-hour snowfalls; the depth on the ground at the beginning, and end, and in the middle of each month, have been observed at numerous stations. Some of the more significant of these data have also been charted. The snow-surveys on our western mountains, greatly extended and perfected in recent years, have provided information concerning the depth and character of the snowfalls at the higher elevations.

The occasional heavy snowstorms of the northeastern sections of the United States not infrequently cause temporary obstruction and delay of railroad transportation, with resulting inconvenience and often considerable pecuniary loss. Various simple methods of meeting these difficulties have been devised, such, *e.g.*, as snow-fences and wind-breaks of growing trees. In many parts of the western mountain areas extraordinarily deep snows are the rule, and there the railroad companies have been compelled to resort to elaborate and very costly methods to enable them to keep their trains running. In removing snows of moderate depths, ordinary snow-plows were first and are still commonly employed. Where snowfalls are very heavy, however, something much more effective than the ordinary plow is needed.

American inventive ingenuity therefore devised the rotary snow-plow, one of the most striking illustrations of the application of human intelligence to the production of a

machine whose sole purpose is to fight one single hostile weather condition. Rotary snow-plows are widely used, and have proved extremely useful, even in heavy snow. A few years ago, when a heavy snowstorm was prevailing over southern New England, the railroad which succeeded in maintaining a fairly regular schedule was the one that had rotary plows ready for service.

To prevent snow from drifting on the tracks various protective devices are employed. One of the most common is the ordinary snow-fence, placed along the windward sides of cuts or at other points where drifting has been observed to occur. Such fences help to reduce the wind velocity, and the snow-drifts form along the lee side of the fences instead of on the track. Anyone who travels on the railroads in the snow-belt is familiar with this type of protection, which is used over many hundreds of miles. The expense of building and of maintaining wooden fences is heavy. Often they are removed in spring and replaced in the autumn. It is, therefore, natural that other forms of wind-break have been investigated and are used. Right here it may be suggested that useful local studies of snow-drifting, of the conditions under which it occurs, and of the most effective method of protection, would supply valuable information to the railroads and would also provide interesting problems for local amateur or professional meteorologists. Most of the observations hitherto made in this matter have been more or less accidental and haphazard. In recent years, both in the United States and in Canada, the use of living trees as wind-breaks has been increasing rapidly. It appears that either conifers or deciduous trees may be used; that the maximum benefit is secured when the trees are about 75 feet away from the track, on the windward side, and are planted in rows about 3 feet apart. Further, those who understand this matter affirm that two rows of conifers or eight rows of deciduous trees when thus planted are equally effective, and that in the long run trees make a more effective as well as a cheaper wind-break than wooden fences. A rather unique

temporary wind-break has been made in Siberia by occasionally piling up the snow itself by means of plows and shovels.

The deeper snows of mountains present more serious difficulties than the generally lighter snows of the lowlands. Especially on the western slopes of the Sierra Nevada and of the Cascade Mountains, where annual snowfalls of over 30 or 40 feet, and in some years, locally of over 60 and even of over 70 feet occur, the snow-fighting campaign is tremendously expensive and arduous, and requires incessant watchfulness. The question whether it is better to tunnel, or to build above the surface and keep the tracks clear, is an important one for the construction engineers to settle. The northern transcontinental railroads of North America, where they cross the western mountains, are protected at critical points by snow-sheds. At other points, where the depths are more moderate, plows pushed by locomotives, or rotary plows, usually suffice. The famous "thirty miles of snow-sheds" on the Overland Route of the Southern Pacific Railroad where it crosses the Sierra Nevada between San Francisco and the East (Blue Canyon-Truckee), well known to travellers, are a striking illustration of the way in which railroads have to meet, and endeavor to overcome, climatic handicaps. These famous sheds are reported to have cost $42,000 a mile over single track and $65,000 a mile over double track. The annual upkeep and renewal cost is about $150,000.[1]

The life of a shed averages a little over 20 years. The danger from fire is so great, especially in the summer, that watchmen are kept constantly on guard, and trains equipped for fire-fighting are kept under steam, in readiness for immediate use. In addition, local engines carry pumps and are followed by tank cars filled with water in case of fire *en route*. In spite of all these precautions, fires have occasionally occurred. In one case, a number of years ago, half a mile of sheds near Summit, California, was burned, with a loss of half a million dollars. These sheds are built to sustain snow 16

[1] Andrew H. Palmer, *Monthly Weather Review*, Oct., 1919, pp. 698–699.

feet deep. If the load is greater, shovelling by hand must be resorted to. Even with all these precautions, sections of the sheds have at times collapsed. The upkeep and replacement of these sheds costs so much that the question of using concrete instead of wood, and of eliminating the sheds altogether, using powerful locomotives and rotary snow-plows, has been actively debated. A dozen or so years ago one of the Southern Pacific engineers told the writer that in his opinion concrete should be substituted for wood because in the long run the former would cost less. The snow-sheds with concrete walls and timber roofs on the Great Northern Railway at the end of the Cascade Tunnel, on the west slope of the mountains, were said to have cost $1,500,000. The question of tunnelling through the higher portions of snow-covered mountains, and thus avoiding the expense of snow-fighting, as well as shortening the running time, is another important problem for the railroads to settle. The initial expense is vast, but in the long run of the years money and time may be saved, and greater safety is secured. The Jungfrau railway in Switzerland was designed to run much of the way through tunnels, under glaciers and snow-fields. The new Cascade Tunnel on the Great Northern Railroad eliminates miles of snow-sheds.

The Pacific Slope mountains, with their phenomenally deep snows, are not the only ones in which winter snowfall makes serious trouble for our railroads. The Rocky Mountains, because of their inland location, have less snow than do the ranges near the west coast, yet from 200 to 300 inches or more a year are indicated for some of their western slopes, as well as for other localities in the same group. The Colorado railroads have had their difficulties, and there have been similar struggles farther north, and even farther south. One occurrence in Colorado a good many years ago was a striking one, and is of historic interest in this connection. The fight to keep the line open lasted from the end of January to the middle of April. Over 700 men were employed. Sixteen engines were used in the effort to plow through the snow. Two locomotives were frozen in for over two months,

and were released on the final day of the struggle. The drifts over the track were 30 feet deep in places, and the story was told that a gang of men on snow-shoes took two hours to find the roof of a snow-shed! It is not surprising that some mountain railroads have been altogether abandoned in winter. In the East there are occasional delays on account of heavy snows in the Adirondacks, the White Mountains and other portions of the northern Appalachian Mountains, but there is nothing nearly as serious as the steady struggle on the western Sierra Nevada and Cascade slopes. E. L. Wells, of the U. S. Weather Bureau, has recently reported an interesting case from the northern plains. Two railroads enter the same town. One is seldom blockaded by snow. The other is occasionally blocked for many days. The cuts on the former are parallel to the prevailing wind direction, while in the case of the latter the wind blows across the cuts. Such a condition emphasizes the importance of a detailed study of local topographical climatology when new railroad construction is contemplated.

Avalanches provide another difficulty in mountains with heavy snowfall. On the steeper slopes avalanches often occur by the hundreds during a winter, perhaps started by a thaw, or by a heavy rain, or even by the jar from a passing train. Snow-slides, when sweeping down onto a railway line, may carry away the tracks or cars, and bury trains, with resulting loss of life. A case comes to mind which occurred in the State of Washington some fifteen years ago. An avalanche engulfed a steel passenger train; broke it in two, and swept some of the cars into a river, with a loss of several lives. On another occasion, this time in the mountains of Japan, a snow-slide overwhelmed a train and over a hundred lives were lost. To prevent such accidents adequate precautions must be taken. The localities especially subject to avalanches should be determined by careful observation. Retaining walls of masonry or concrete, or chutes, or piers may be built; or stakes may be driven in at the danger points. Yet, as R. M. Brown has well expressed it, "a surface road remains a surface road, subject to the attack of moving snow."

The snow-removal bill of the American railroads within the snow-belt is almost unbelievably large, and goes up or down with the varying amounts of snowfall in different years. Innumerable statistics could be given, but two examples must suffice. On one of the New England railroads a few winters ago snow removal after one heavy storm cost $25,000 for each inch of snow that fell. This sum included the actual labor of removal, and also the extra motive power, the cost of demurrage on freight cars, the extra wages paid for overtime, and other expenses. According to the Bureau of Railway Economics, the Class 1 railroads had a net operating income of $63,289,297 in February, 1926, as compared with $65,151,053 in the preceding February. The decrease in net income occurred in spite of a gain in gross revenues of more than $5,000,000. According to railroad men, this decrease was directly traceable to the large maintenance expenditures incidental to the snowstorms of February, 1926. These storms, it was estimated, cost the railroads between $5,000,000 and $6,000,000. Even a relatively moderate snowfall may temporarily delay traffic and involve considerable expense. The difficulty of budgeting the cost of snow removal is the extreme variability in the depth of a winter's snows. A single heavy storm may cost as much for removal as do all the storms of some other year. Hence there is no certainty in advance. If the sum budgeted is sufficient to take care of an average year's expense, it will, of course, be more than enough to provide for a winter with a minimum snowfall, and be far too small, it may be by millions of dollars, to fight the snows of a heavy winter. Another item of expense resulting from snow is the added weight of cars when they are carrying a load of snow on their roofs. This may become a matter of considerable economic importance, especially in the case of a long train of freight cars. The added weight may, at times, be over a ton per car.

(*b*) *Ice Storms.*—Sleet and ice storms, the latter officially known as glaze, are closely associated with snowstorms in the eastern United States. It is often difficult to forecast

snow because sleet or glaze may occur instead. When rain drops fall to the earth's surface, and freeze on coming in contact with solid objects at temperatures below freezing, glaze or an ice storm results. Telegraph, telephone and trolley wires; trees, sidewalks and streets are then covered with a coating of ice. When the accumulation of ice is heavy there is likely to be great damage. Transportation becomes difficult and dangerous by reason of slippery rails and streets. The railroads over much of the country never have to contend with ice storms. The broad central belt, reaching from west of the Mississippi eastward and northeastward to the Atlantic is the region especially subject to them. According to the late Professor H. C. Frankenfield, an average number of over three ice storms a year occur in New England, the Middle Atlantic States, the great interior valleys and the southern Lake region, while the average rises to over six in New Jersey, eastern Pennsylvania, southern Illinois and eastern Missouri. Severe ice storms are less frequent in the southern States and in the northernmost States and southern Canada, the intermediate districts being the most subject to them. It has been noted that the trees in northern New England are more symmetrical and shapely than those in southern New England, because the latter suffer more from ice stoıms. The months of maximum frequency are from November to March. It is probably the fact that small portions of most of the United States and Canada have occasional experiences with glaze during the run of their winters. Even the northern Pacific coast is not wholly exempt. A report from Seattle, Washington, notes that that city's "chief affliction in the colder months (is) frequent rainfall, attended sometimes by the formation of glaze that makes travel difficult in this hilly city."

Railroad and electric car service is inevitably badly crippled in a severe ice storm. Slippery rails; overloaded and broken wires; telephone and telegraph poles and trees snapped off and lying on the tracks; frozen switches and signals, are among the ordinary consequences. In November, 1921, an unusually severe ice storm in parts of Massachusetts and of

central New York, lasting, with interruptions, about three days, deposited 4 inches or more of ice on the ground in places, and as much as 2 inches or so on wires, trees, and other objects. This belt of glaze lay between a district farther north where snow fell, and one farther south where it rained without any accompanying ice formation.

The situation is bad enough for steam roads. It is worse for electric lines. Overhead trolley wires are easily coated with ice. In the third rail system adequate protection is difficult or impossible; contact is lost; service is interrupted. In his efforts to overcome these difficulties man has resorted to various devices. Under-contact rails; spraying with calcium chloride; trolley wheels provided with claws to rake off the ice; heating of wires by means of a powerful current—these and other methods have been used, but most of the difficulties still remain, and a severe ice storm is sure to be a serious handicap. Slippery rails, at their worst during glaze, are also caused by snow or rain. Such conditions not only interfere with the maintenance of a regular schedule, but increase the danger of accidents. When the rails are sanded, friction and safety are greatly increased, but on the other hand locomotive tires and car wheels are subjected to extra wear and if such conditions are often repeated, the life of the wheels is shortened, with added expense for upkeep.

(*c*) *Low Temperatures.*—There is no lack of information concerning the temperatures likely to occur at any time of year in any part of the United States. For the railroads, winter cold makes more trouble then summer heat. And as to the amount of cold there are available not only the familiar mean annual and mean monthly temperatures, but, what is far more important, the mean annual and mean monthly extremes of temperature; the absolute minimum ever observed; the numbers of days on which the lowest reading was below freezing ("frost days") and of days with temperatures continuously below freezing ("ice days"), etc. Preparing for the winter's cold is no longer the haphazard campaign it was in the early days of railroading, when meteorological observations were few, and wholly inadequate.

The difficulty of making steam in very cold weather is well known, but perhaps comparatively few persons realize that low temperature increases the rolling friction of trains and the greater density of cold air increases the resistance which the atmosphere offers to moving trains. So much difficulty may result for these reasons that the train tonnage may have to be cut down, or more motive power may have to be supplied. The freezing of turntables, signals and switches is one of the handicaps that the railroads have to meet in winter cold, especially if rain, or snow, or glaze, are associated with the cold. Even a small piece of ice may make a switch inoperative. To thaw out frozen switches, etc., kerosene, gasolene, steam, hydrocarbon torches and electrical heating appliances are used. The added cost of labor is a considerable item. A new method of fighting frozen switches was recently reported in use in the Hudson River Terminal yards of one of the great eastern railroads.[1] A special steam plant, costing $2000 a year to run, is operated for this purpose. Regardless of weather conditions, this plant used to be operated from December 1 to March 31, but experience showed that the frequency of snowfall was not great enough to warrant continuous operation. Hence, as about six hours are needed for getting up steam, the plant is not started until the weather forecast indicates probable snow. A considerable saving of money has thus been effected.

The freezing of coal, or sand, or ore, in open freight cars is another consequence of severe cold, and steam sheds have in some cases been provided for thawing out coal in hopper cars. Although the incident must have happened often, but one case has come to hand of the freezing of the water in a locomotive tender. This happened, rather surprisingly, in eastern central France, when the engine of a Côte d'Azur express broke down near Dijon on that account. When cold is combined with moisture from rain, melting ice or snow, in a dirt roadbed, the resulting ice may heave the track, causing the rails to spread and providing favorable conditions for an

[1] *Science Service Notes.*

accident. Stone ballast counteracts this danger, but adds to the expense. When heavy rains are combined with the added water supply from melting snow, as happens especially in spring, dirt ballast is likely to be washed out, and the tracks may be undermined. Here again stone ballast, combined with an adequate drainage system, is a solution, although not always a complete one.

Severe cold causes contraction of rails, girders and other metal work, whereas high summer temperatures cause expansion. The allowances for these effects have, of course, been carefully calculated, so that buckling due to expansion in hot weather is now a very rare occurrence. An interesting case of an accident in England which resulted from expansion of the rails during a spell of high temperatures in spring comes to mind. On March 26, 1907, a passenger train on the Northeastern Railway left the rails near Felling, soon after noon on a warm sunny day. The investigators for the Board of Trade reported that the derailment was caused by expansion of the rails because of "unusual heat." The trackwalkers had not been along the line as is customary in the summer months. It was held that the derailment was "accidental"; resulted from an abnormal temperature for that time of year, and 'could not reasonably be expected by the men in charge of the section.' Again, in bridge construction, if the iron or steel girders were solidly embedded in concrete or stone piers and abutments, in any region where the temperature ranges are fairly large, expansion and contraction would rack such metal work and might easily lead to grave consequences. In all such calculations the most accurate and most complete temperature data must be used. Another effect of cold was carefully studied in the case of Bessemer steel rails, which were often broken in the severe cold of our northern winters. This problem was solved by the use of rails made by the open-hearth process.

Numerous additional examples might be cited did space permit. One case, of a rather special local significance, may be mentioned. When the Hudson River is filled with floating

ice, large cakes of ice are blown by the strong westerly winds onto the eastern bank. To protect its tracks, the railroad that skirts the east bank of the Hudson has built a more or less continuous and very substantial barrier in the form of wooden fences, well braced on the land side. When the wind is unusually strong, ice is occasionally driven up the embankment and the inclined face of the barrier, but falls before reaching the tracks. This fence, which extends for miles along the river, and is daily seen by thousands of travellers during the winter months, also serves as a snow-fence.

During railroad construction in high latitudes with harsh winters, the cold, snow, ice, and frozen ground not only delay the work but often stop it altogether. The Trans-Siberian Railway, traversing climates with very severe winter cold, presented many interesting problems. Great difficulty was experienced because of frozen ground, spring thaws, upheaved tracks and interrupted work. In fact, the whole story of construction and operation of this railroad is a fascinating chapter in engineering from the meteorological standpoint. Temporary rails were laid across frozen rivers and on the ice of Lake Baikal, and ice-breakers and car ferries were also used. But the snows of the semi-arid Asiatic continental lowlands are light, and the serious handicaps resulting from heavy snows were not an ever-present problem there.

(*d*) *Wind.*—High winds, characteristic of winter rather than of summer, have occasionally blown cars off the track at exposed places, and the maximum possible wind velocities must always be taken into account by structural engineers in building railroad bridges, trestles, and the like. Further, the resistance caused by high winds has often to be taken into account by yard masters when trains are made up. The weather forecasts are constantly put to practical use. When unfavorable conditions such as high winds, or deep snows, are forecasted, the number of tons' load per freight engine is regulated accordingly. A very interesting case, reported in an English meteorological magazine, may be mentioned of one railroad's precautions against high winds. On the west coast

of Ireland a short piece of narrow-gauge railroad forming part of the West Clare Railway between Ennis and Kilkee is exposed to heavy westerly gales directly from the sea. There is probably no other line in the British Isles similarly exposed. Trains were several times derailed by the wind. Shelter banks thrown up on the windward side of the line proved ineffective and expensive. After various other methods had been tried, a pressure-tube anemometer was installed by means of which warnings are automatically given when the wind attains dangerous force. At such times a bell rings in the house of the station master. A first warning sounds when the wind velocity reaches 65 miles an hour. At this, a certain definite weight of movable ballast (2400 lbs.), which is kept for this purpose at the stations, is placed on each car of the train, this being sufficient to prevent an overturn. When a second warning is rung the wind is blowing 85 miles an hour, and trains are stopped until the gale subsides. Near Ulverston, England, at the western end of a viaduct there used to be, and very likely still is, a gauge for determining wind velocities dangerous for the passage of trains across the viaduct. Boards, kept in a vertical position by springs, have their deflection from the vertical recorded by means of a pen on a chart which is driven by clockwork. When the wind pressure reaches 32 pounds per square foot, an electrical contact is made, and bells ring in signal cabins at both ends of the viaduct. Trains are then held until the force of the wind moderates. On February 20, 1907, the Irish Mail train from London to Holyhead could not take water from the "pick-up" troughs because the wind had blown all the water out. A stop for water had to be made at a station. In the United States derailments due to gales have seldom been recorded. For one thing, our passenger and freight cars are heavier than those of Europe. There is on record at least one occasion on which a train on the Mt. Washington (New Hampshire) railway was forced to abandon its trip because of a heavy gale.

(*e*) *Heating Cars.*—The problem of caring for perishable

freight is one in which the weather factor plays a critical part at every stage, but is not considered in the present paper. Reference may, however, be made to what has been, and what has not been, done for the comfort of passengers. The heating of cars in winter has from the beginning of railroading in the United States been a necessary measure for the comfort and health of the passengers. Yet those who travel will agree that our present methods are far from satisfactory. Our cars are usually superheated, and the cooling process, by means of opening ventilators, is often inadequate and usually uncomfortable. Completely new devices for heating our trains adequately but not excessively in cold weather have long been needed and are fortunately reaching the stage of introduction.

2. *Some Weather Handicaps of Summer.*—The more peaceful weather of summer, without the handicaps of cold, and ice, and snow, brings to our railroads far less "weather-fighting," and on the whole a much more simple and regular routine. There are, however, certain hostile conditions inherent in our differing types of climate which the railroads have to meet as best they can.

In the Ohio and lower Mississippi drainage areas floods of greater or less severity are normal, especially when heavy and prolonged warm rains occur and when deep snows still lie on the mountains in late winter and early spring. Such floods, if a combination of the controlling factors results in unusually deep waters, carry away bridges and culverts, wash out tracks, and seriously interrupt railway service, at times even carrying away cars. The flood of 1903 in the Mississippi basin cost one railroad alone $1,000,000. The Mississippi flood of 1913 destroyed railroad property to an estimated value of over $15,000,000. Floods being more or less normal in this great interior region, with an especially disastrous flood occurring at intervals through the run of the years, it would seem to a layman that more and better provision might be made by the railroads to meet these inevitable difficulties. The Mississippi lowland is by no means the only portion of the United States

in which floods occur. The mountain areas are naturally subject to them, and in recent years two districts as far apart as northern New England and southern California have experienced severe and expensive floods. To know in advance the meteorological conditions that produce floods in different parts of the country; to use the available rainfall and snowfall data in order to know as definitely as possible the extreme conditions for which provision should be made, is merely to practice ordinary precaution.

Bridges across rivers must be firmly anchored on piers that cannot be carried away in the greatest floods. If built across what are usually dry canyons in the mountains of arid regions, it should be remembered that sudden cloud-bursts are characteristic of such localities, bringing downpours that may fill these narrow valleys to a depth of many feet of water in a very short space of time. One case is personally known to the writer, and the locality has been visited by him. The first railroad bridge across a certain canyon, usually dry or with a shallow stream flowing through it, was anchored on piers placed on the bottom of the gorge. This was completely carried away after a cloud-burst. A later bridge was of the suspension type without supports on the canyon floor. This second type of bridge should have been built originally. Many low-lying deserts are at times subject to floods from overflowing rivers supplied by unusually heavy rains on mountains within or on the borders of the desert. In such cases tracks laid directly on the surface of the sand, especially when near rivers, may be flooded and washed out. Occurrences of this sort have not been unknown in the history of railroading in the southwestern United States. A comparatively recent instance comes to mind in which a considerable section of track had been laid too close to a river. Because of the damage caused by occasional floods, the line was moved to a new location higher up. In our Southwest, rains of the cloud-burst type are very destructive to roads and railroads. These downpours have tremendous erosive power on slopes unprotected by vegetation, and not in-

frequently cover fields and low-lying ground with thick layers of sand and mud. The railroads have been forced to protect themselves as best they may against these floods. Drainage channels several thousand feet long lead the flood-water to specially protected culverts. One of the southwestern railroads has built massive concrete walls at critical points. Yet in spite of all these protective measures, every now and then sections of the track are likely to be washed out. It is one of the anomalies in climatology that the economic losses from heavy rains are often greatest in the most arid regions.

The heavy summer rains especially characteristic of mountainous districts often cause landslides, notably on the steeper slopes of the deeper valleys. The right of way may be buried under immense quantities of earth and rocks, involving heavy expense and interruption of traffic. A study of the local topography, geology, soils, vegetation, rainfall and other controls is essential in planning the most effective protection against landslides. This protection may consist of retaining walls or other masonry abutments; of trees or grass planted at critical points on the steeper slopes; of culverts and of other devices.

On deserts, and in dry seasons, especially in summer, there is great difficulty with blowing sand. Sand dunes and blowing sand often move onto the track under the influence of the wind; cause deep drifts, and must be removed by shovel or plow. To obviate this difficulty, fences are built as in the case of blowing snow, and grass or desert shrubs are planted on the dunes. A network of plaited brush or faggots has also been used as a covering over the loose sand. Blowing sand greatly increases friction on all moving parts of locomotives and cars, and shortens the life of these parts. Similarly, railroads which follow sandy valley bottoms or the sandy shores of lakes must be protected in similar ways. Many illustrations could be given. Two of the most familiar, because noticed by thousands of travellers every year, are the board fences used along the railroads that follow the Columbia River valley, and also in the case of those on the

south shore of Lake Michigan. Coarse grasses like those seen on the dikes of Holland, as well as acacia trees, have been used in California as protection against blowing sand. Travellers across the southwestern "deserts" of the United States have seen the stones piled up around the bases of the telegraph poles to protect these against being worn through by sand-blasting. In constructing and operating railroads in deserts there is always the problem of an adequate water supply, first for the laborers engaged in construction and later for the locomotives and the ordinary uses of the employés. Water is variously supplied from local water-courses; from ground-water; by means of tank-cars and also by distillation. Travellers who have crossed the Arizona desert by rail may remember *Sunshine* station—indeed well named—where tank cars with water are brought down the line from a distance of 50 miles or so. The water flows from the bottom of the tanks into a sluiceway, and thence into a large underground tank from which it is pumped to an elevated tank, above the track, for the use of the locomotives.

The summer heat of continental interiors, and especially of the extreme type of continental climate found in deserts, when combined with drought conditions, results in drying up buildings and other wooden structures, greatly increasing the fire risk. During dry spells even in regions with generally sufficient rainfall, forests along railroad rights of way are often set on fire by sparks from locomotives and much property, in the form of trees and of buildings, may be destroyed. Droughts, either in winter or summer, but more frequent and economically more critical in summer, have frequently curtailed operations on electrified railway lines by reducing the power available for hydro-electric plants.

Spring and early summer are the season for tornadoes, and railroads that traverse the so-called "tornado belt" of the eastern United States are exposed to the excessive and irresistible violence of tornado winds, whose velocities may occasionally reach 300, 400, perhaps even 500 miles an hour. There is no protection against such force, but fortunately

tornadoes are relatively rare, are short-lived, and have a very narrow path of destruction. Hence the danger that any single train will be in the path of a tornado is almost incredibly remote. However, seven out of eleven sleepers on one of the limited transcontinental trains were blown off the track in a tornado a dozen or so years ago. Another incident in connection with a tornado occurred in Missouri. A train was travelling eastward at a moderate speed when the locomotive engineer saw a tornado coming up the track behind him, *i.e.*, moving, as such storms do, towards the east. The engineer put on full speed, and slowly pulled away from the storm which, after a few miles, left the track.

The whole problem of conserving ties has given rise to much investigation. Moisture is obviously the chief agent in shortening the life of a wooden tie, and various processes have been tried in the effort to overcome the effects of moisture. Solutions of zinc creosote and of other chemical compounds, *e.g.*, zinc chloride, have been used. Plants for treating ties chemically have been built by certain railroad companies. In spite of the difficulties and the expense involved in the use of wooden ties, and in their frequent replacement, metal ties have been but little used in the United States.

The growth of weeds along the right of way of earth-ballasted roads has sometimes given difficulty in parts of the temperate zone, although the problem is much more serious in the wet tropics. One of the great transcontinental railroads has used gasolene weed-burners, and the water of Great Salt Lake, sprinkled over the road-bed from tank cars, has been found to serve well as a weed-destroyer.

Another response of railroad operation to seasonal weather is found in the volume of freight carried. The business of the railroads is largely seasonal, and depends to no small degree on the special character of each season. The time when the railroads are overburdened is after the harvest of the staple cereal crops. Then, in the great agricultural districts of North America, the immense crops can only with difficulty be handled. "Moving the crops" is one of the railroads' most important functions.

It is surprising that so much thought and ingenuity have been expended on refrigerating cars carrying perishable food in hot weather, and practically no attention has been given to the matter of keeping passengers cool and comfortable. The southern transcontinental railroads of the United States, which traverse the hottest and dustiest part of the desert, lose passenger travel in summer because many people prefer the more northerly, usually somewhat cooler and also less dusty journey. Partly to offset this tendency, these southern roads have, very properly and honestly, made it a special point to advertise the natural scenic attractions along their routes. It has, however, for years seemed to the writer that it would have been a wise move for these same railroads to paint the cars of one or two of their limited trains white, and then to advertise extensively the advantages of travelling in cars which reflect much of the sunshine. Our ordinary passenger coaches and sleepers are all painted in dark colors, and are therefore excellent absorbers of solar radiation, as anyone knows who has ridden in them in hot summer weather. White trains are well known in certain parts of the tropics, and are probably most familiar to Americans because of their having been seen in Egypt, whose climate is by no means of the typical "hot-house" character. At one time a well known Florida express train was painted white, and became known as the "ghost train." The writer brought this matter of painting trains white to the attention of one of the officials of an important railroad which crosses the Southwest, but the idea did not strike that official favorably because of the expense of having the repainting done at frequent intervals.

All that our railroads have thus far done to make summer travel more comfortable is to install electric fans, which do not cool the air! With the recent rapid developments in the way of producing "artificial weather" in factories, theatres, and other buildings, it was inevitable that steps would soon be taken to cool our passenger cars in summer. So far as the writer knows, but one railroad has as yet actually installed apparatus for "manufacturing" weather in its cars, and that

is one of the great eastern roads. On one of the dining cars of this company the necessary mechanical devices have been installed which provide both clean and cool air. Free air is admitted, but it is cleared of its dust, soot and cinders by means of a mechanical filtration system, and at the same time is brought to a comfortable temperature by passing it over coils cooled by an ammonia compressor system. In a test made on a regular train the air temperature was reduced from 93° to 70° in 20 minutes by automatic control. In addition, the relative humidity is also controlled, within reasonable limits. Naturally, the double windows are kept closed, and the doors are opened only when used for entering and leaving the car. "Artificial weather" will, without doubt, be increasingly used on our trains during the hot season, and thus will man once more show his mastery over weather conditions, which he finds uncomfortable or harmful. But in this case as in others in which man fights weather which he dislikes, the mastery, be it more complete or less complete, is over limited and confined bodies of air, not over the free air outdoors.

3. *Some Climatic Handicaps in the Tropics.* (*a*) *General.*—One of the rather natural misconceptions of a layman is that the climates of the whole tropical zone are in all respects alike. This is by no means the case. There are, in reality, three logical subdivisions of that zone; the equatorial belt, the trade wind belts, and the monsoon belts. In each of these there are modifications due to oceanic and to continental influences. Further, the effect of altitude is so important that another subdivision should be added to include mountain climates. All parts of the hot zone are not equally disagreeable or hostile, so far as occupation and exploitation by the white race are concerned. The tropical zone includes extended deserts over the continental areas in the latitude of the trade winds; immense expanses of damp forests and jungles and swamp-land in the vicinity of the equator; fertile islands, refreshed by cool steady winds from the ocean; great grasslands with one season of drought and dust and

another season of dampness, of rains, and often of floods; the monsoon districts, with their climatic control alternately that of the wet monsoon and of the dry monsoon; the mountains and plateaus with their lower temperatures, and if rising high enough, carrying snow the year around, even on the equator.

In this variety of climates, railroad construction and operation have had to meet widely varying problems; interesting, novel, taxing the ingenuity of the engineering profession. Time fails for more than brief reference to a few of these climatic handicaps, and to the ways which have been used of meeting them.

In the Tropics as a whole there are certain problems with which the railroads have to struggle. Most of these are similar to those met with in temperate latitudes, but are often greater, and some of the problems are quite different. Take, *e.g.*, the geological processes and their relation to engineering. In the heavy warm tropical rains, often continuing more or less steadily throughout the year but in many latitudes interrupted by a longer or shorter dry season, chemical decomposition and decay are far more active than in cooler latitudes. This factor is of critical importance in laying out and in maintaining roads and road-beds. Further, the erosive action of the heavy rains and of running water where there is no cold season to bring snow, or to lock up the streams by freezing them, presents a very different problem from that met with in colder climates. The geological work of tropical organisms is an additional factor which needs consideration. It was pointed out some years ago by Warren D. Smith, then connected with the Division of Mines in Manila, that handbooks and formulæ prepared by engineers familiar with temperate zone conditions are of little use in the Tropics. They may even be dangerous guides.

Where the rainfall is heavy, and the relative humidity is high throughout much, or most, of the year, there the deterioration of ties, the destructive effects in the case of all

wooden construction, and the rusting of iron are naturally most marked. Hence the necessity of a careful selection of the most durable woods, such as lignum vitae, camphor wood, etc., and of their treatment with creosote or some other preservative. The expense of such treatment, and of the frequent renewal of ties and other wooden materials, is one of the financial burdens which railroads in the damp and rainy Tropics have to bear. In many cases the life of ties, etc., has been a matter of but a few months, or of a year or so at most. It has been reported that on a certain Central American railroad some bridges of Oregon pine became unsafe soon after they were built; that the life of the ties was less than one year, while telegraph poles rotted in six months. One of the outstanding difficulties throughout the history of railroad construction and operation in the Tropics has been with the health of the laborers and employees. Fevers of many sorts, cholera, and other diseases have incapacitated thousands from work, and have resulted in a high death rate. With the progress of modern sanitation; of proper hygiene; of various protective methods, medical and otherwise, and of hospitals, this situation has very greatly improved at the expense of a great outlay of money. The handicaps resulting from actual illness have been combined with those due to the heat and moisture. Work must often be suspended during the hottest part of the day. On the Madeira-Mamoré Railroad in Brazil night construction was necessary, but brought new difficulties because of mosquitoes. On one of the African railroads a special system of lighting was devised to facilitate night work. A freight car was used as a lighting plant with a long arm projecting out over the track from a tower built at one end of the car. Two search-lights were placed at the end of this arm, and other lights were placed at intervals along it. Thus light was shed on the track under the arm, and was also projected ahead where the road-bed was being prepared. Work is also interrupted by heavy rains. All this involves loss of time and additional expense. Almost everywhere the problem of securing the most efficient

kind of labor has been acute. The natives of the immediate vicinity; those from other parts of the country; imported labor, from China; India and elsewhere—all have been tried, with varying degrees of success or failure.

To make travel more comfortable in tropical climates various methods have been employed. Tanks containing ice and fans to keep the air in circulation have been used in Egypt, and screens hung at the windows and thoroughly wet while the trains are stopping at stations have long been familiar in India.

(*b*) *The Equatorial Belt of Heavy Rains.*—Over the greater part of the equatorial belt we find hot, sultry, cloudy weather with frequent heavy rains more or less continuously through the year but with one or two drier seasons when the sun is farthest north or south. The superabundant vegetation in the dense forests, which cover immense stretches of the equatorial lowlands, is highly unfavorable to human occupation. Work on railroads is always greatly interfered with during the heavy rains, if not interrupted altogether. The downpours wash the sides of freshly-made cuts. The roadway is constantly being overgrown. To keep down the rapidly-encroaching vegetation men used to be kept at work cutting down the weeds, underbrush and young trees. One of the writer's memories of the Panama Railroad in 1898 is of the gangs of men walking along the right of way, cutting down the vegetation with *machetes*. In recent years various other methods have been used, such as spraying at intervals with a strong chemical poison solution thrown out from tank cars. This method has proved less costly and more effective than cutting by hand labor. Ties and trestles rot quickly, or are destroyed by insects. Special kinds of wood, or even metal ties, have been used. Fevers and other diseases common in the rainy season of the Tropics have been a serious handicap, and floods and landslides add to the difficulties. The sultry heat is ever-present.

Curious complications arose in the employment of several different castes of laborers during the construction of the

Uganda Railway. For each of the four castes among the Indian laborers a separate water tank had to be provided. If the water in one tank gave out, that particular one had to be sent away, to be filled, although the remaining three tanks were full. On the Uganda Railway the early use of metal cars in order to defy wood-boring insects; the installation of deep ventilators protected by wire gauze to keep out mosquitoes and other insects, and the use of green window glass to give protection against the glare of the sun, are obvious illustrations of protective devices.

(*c*) *The Deserts.*—On the deserts of the Tropics, as in those of the temperate zone, a definite series of problems presented themselves which have called for ingenuity, patience and money. These problems are similar to those already referred to in the case of temperate deserts: the warping and splitting of ties and woodwork; the danger of fire and sometimes the need of fire patrols; the discomfort and added friction resulting from the dust; the difficulty of providing water and fuel; the incessant struggle against blowing sand. The aseptic air of the tropical deserts has usually kept down the disease and death rates, but on the other hand the excessive heat has reduced the working hours and increased the construction cost. Lack of fuel means transportation of coal, wood, or oil, often from considerable distances. In the early days of railroad construction across tropical deserts this was a very serious handicap, now greatly lessened with the development of modern means of transportation. There are cases on record of the use of a poor local fuel, like sheep or llama dung. All lumber must be brought from rainier climates.

Along the margins of these deserts, as in the temperate zone deserts, occasional cloud-bursts and floods occur which sweep away bridges and tracks; cover low-lying districts with water, and may interrupt operations if the tracks are laid directly on the surface. The question of saving time and money by laying the tracks directly on the surface, and thus risking overflows at certain critical points, or of building more

expensive bridges, viaducts, embankments, and culverts is one for the engineers to settle. It is not always an easy one, and the answer involves a study of the rainfall and flood conditions over as long a period as possible. R. M. Brown has reported that "the Sind authorities in 1861 were willing to foster the belief that in regions like Sind, where rainfall is so uncertain, of such short duration and at the same time so heavy, the safest and most economical way of crossing a flat plain is to lower the track to the surface and allow the water to flow over the well-ballasted bed at will. The annoyance of a subaqueous road-bed was endured five years."

(*d*) *The Grasslands.*—Between the equatorial forest belt on the one side and the trade wind deserts on the other there is a transition zone of generally moderate or light rainfall. Here the forests are replaced by an intermediate belt of more or less open, grassy country, often with scattered tree growth, known as the savannas. Here the complete yearly cycle brings a rainy season in the high-sun months and a dry season in the low-sun months. The length of these seasons, and the amount of rainfall in the wet season, depend on the latitude, but are also controlled by distance from the ocean, topography, and other minor factors. Savannas are found very typically developed in Africa and in South America, both north and south of the equator. In Africa they include the Sudan; in South America, the llanos of Venezuela and the campos of Brazil. Australia provides the "downs." During the dry season, the problems of the railroads are similar to those met with in the adjoining deserts. In the rains, on the other hand, the problems are similar to those in the regions of heavy tropical rains near the equator. As the sun moves north and south, and the seasons change, so the railroads' problems change. In some respects, therefore, the marginal regions of the savannas offer more climate obstacles in railroading than do the equatorial forests and the deserts. The advantage in the savannas is that the duration of each type of problem is shorter. In the monsoon districts, with their single rainy and single dry season, travel and transportation depend closely upon the season.

(*e*) *Tropical Mountains.*—Mountains are found in all parts of the Tropics; in the deserts, the grass-lands, the equatorial forest belt. They naturally have heavier rainfall than the lowlands, and therefore present special difficulties in the way of floods and landslides. Mountains in the arid regions of the Tropics, as in the higher latitudes, are especially subject to the cloud-burst type of rain, however much such a phenomenon may seem out of place in such a climate. Bridges and embankments and road-bed must be constructed with this fact in mind. In many tropical mountain areas landslides have been a serious and continuing handicap in the rainy season, as, *e.g.*, in the Himalayas, in northern Venezuela, the Cordillera of South America, and elsewhere. In the higher passes over certain mountains diurnal winds are encountered of such velocity that travel may become dangerous. In the South American Cordillera, railroads have been built up to greater altitudes than elsewhere. And there "mountain sickness" proved a serious handicap, incapacitating the laborers and greatly curtailing their hours of work and their efficiency. In the case of the famous Oroya Railroad in Peru (maximum elevation above sea-level, 15,665 feet), a road concerning which the writer has personal knowledge, efforts were made to select engineers and laborers who were more or less immune to the effects of the diminished oxygen pressure at great altitudes. The employees of the Oroya are given time off to be spent at low levels, for the purpose of recuperation. On a trip made by the writer over the Oroya Railroad, from Lima to Oroya (1898), practically every passenger on the train was ill. Even the conductor, a Scandinavian-American, who was making his first trip in three months, suffered from nausea and weakness.

The São Paulo Railway, between Santos and São Paulo, in Brazil, furnishes a wonderful illustration of the engineering difficulties in the case of heavy rainfalls on the slopes of tropical mountains, although this locality is just on the southern Tropic and the handicaps resulting from very dense vegetation are not present. This journey, first made by the

present writer in 1908, takes the traveller by an inclined plane cable road from sea level to an altitude of a little less than 3000 feet in a horizontal distance of about five miles across the Serra do Mar. The rainfall on the seaward slopes of the Serra is very heavy, well exceeding 100 inches a year. The engineers of the São Paulo Railway have an incessant struggle to keep the road in repair. The trip from Santos to São Paulo is well worth taking as an illustration of marvellous engineering work in the face of heavy odds. The whole face of the mountain is in places walled up with brick and masonry, and brick or concrete drainage ditches and canals have been built up and down and across the slope in all directions. One of the engineers of the road made the statement that his own ambition, and that of his colleagues, was to know what becomes of every drop of water that falls on the seaward slopes of those mountains! An old railroad line, the one first constructed and then replaced by the newer one at a better grade, was kept open and ready for use in emergencies, in case the newer line was washed out.

Addendum.—On re-reading the foregoing pages the writer is very conscious of the incompleteness of his treatment of a very interesting, and, as it seems to him, an important line of investigation. Inadequate as the presentation is, and few and scattering as are the illustrations cited, this paper will have served its purpose if it creates an interest in the problems here referred to, and especially if it should stimulate someone else to undertake a thorough investigation of the whole question of the railroads' struggle against the weather, here only superficially outlined.

DIFFUSION PROCESSES IN NON-LIVING AND LIVING SYSTEMS

By M. H. JACOBS

(*Read February 6, 1931*)

Of all the phenomena dealt with by the physiologist there is none more universal in its occurrence or more fundamental in its importance than diffusion. Every cell, in every organism, during every moment of its life, is dependent upon diffusion for its continued existence. By diffusion it receives needed materials from its environment; by diffusion it distributes these materials within its own boundaries; by diffusion it eliminates useless and harmful substances produced by its own activities. Without this process life is inconceivable.

Now, physically considered, diffusion has certain characteristics that seem out of keeping with such a complicated, well-ordered and adaptive mechanism as a living organism. In the first place, it is essentially a phenomenon of chance. Suppose, for example, a molecule of oxygen is brought within a millimeter of a living cell and started towards it with the velocity of a rifle bullet, which is, for it, an entirely usual velocity. How long will it take to reach the cell? Nobody can say. It may do so in a fraction of a second; it may fail to do so in a year. During every second of its travels it will collide with some billions of other molecules. Every time it does so its velocity and direction of movement will suffer a change. The path which it will follow, though determined by physical laws, is utterly beyond the powers of human prediction. It forms perhaps the best example we know of the operation of the laws of "pure chance."

It is true that what is highly uncertain for a single molecule becomes increasingly certain as the number of molecules is increased. Where an enormously large number is involved,

as in any ordinary diffusion process, the problem becomes a statistical one and, as such, can be dealt with with a very high degree of certainty. But even here the influence of chance is still seen in the slowness of diffusion in any given direction in comparison with the actual rate of movement of the diffusing molecule. For example, suppose a million sugar molecules are started at the bottom of a cylinder filled with water. By an application of the laws of probability and with a knowledge of certain experimentally determined facts concerning the behavior of the diffusing substance, we may calculate with considerable accuracy what the distribution of the molecules will be at the end of, say, an hour. It will be something like this:

Vertical distance from the bottom of the vessel	Number of molecules
0–1 millimeter	456,890
1–2 millimeters	319,210
2–3 millimeters	155,800
3–4 millimeters	53,100
4–5 millimeters	12,640
5–6 millimeters	2,100
6–7 millimeters	240
Over 7 millimeters	20
Total	1,000,000

During the hour while this arrangement is being established, each molecule will have travelled an actual distance of perhaps several hundred miles, but because of the erratic nature of their paths a large proportion—*i.e.* almost half of the total number will have progressed less than 1 millimeter in a forward direction. Only 20 in a million will have gone as much as 7 millimeters. The average distance of all of the molecules from the bottom of the vessel after each of them has made a journey of some hundreds of miles will be just a little over 1.3 millimeters. This appears to be a very inefficient method of transportation. But the inefficiency becomes enormously greater as the distance is increased. To diffuse an average distance of 1.3 cm. instead of 1.3 mm. would require not 10 hours but 100 hours, the time increasing as the square of the distance. To travel, on an average, a

forward distance of 1.3 meters, which is something less than the length of the human body, would require approximately 110 years; to travel the length of a whale or 13 meters would require 11,000 years.

In view of the great slowness of transportation by diffusion where any considerable distances are involved, it may seem surprising that the method has been so extensively used by organisms. It should be remembered, however, that as the distances are shortened the inefficiency of the process falls off at a very rapid rate. Indeed, the rapidity of diffusion processes where distances are small is as startling as its slowness when the distances are large. The square-of-the-distance law works both ways. For example, if an hour is required for an average progress of 1.3 millimeters, the length of a very small insect, the time required to transport the same amount of material 1.3 microns, the order of magnitude of the diameter of many bacterial cells, would be 1/300 second. For a distance one-tenth as great, which probably exceeds the thickness of the plasma membrane, through which diffusing molecule must pass to enter living cells, the time would be 1/30,000 second.

Diffusion is therefore either a very slow or a very rapid process, according to the way it is looked at. Where the distances are sufficiently small it is much faster than transportation by convection, as, for example, by the blood stream. Within the body of any organism like man there is a general and very interesting engineering problem, namely, how to combine the processes of convection and diffusion in such a way as to obtain the maximum advantages from each. If time permitted, striking illustrations could be given of the successful way in which this problem has been met in, for example, the spacing of the capillaries, the peculiar shape of the red blood corpuscle, etc.

The second point about diffusion that seems at variance with the fundamental characteristics of life is that it is essentially a process of dispersion and disorganization, whereas life depends upon the opposite tendencies of collection and

organization. Indeed, in a sense, diffusion is the greatest enemy of living matter. It is only by holding it in check that living cells may for a time exist; ultimately, in the case of every organism there comes a time when the forces of diffusion triumph and its materials are hopelessly and irrevocably dispersed.

Many examples might be given to show the way in which diffusion processes are temporarily held in check by organized matter; I shall mention one having to do with a type of cell in which I have been especially interested for some years, namely, the mammalian red blood corpuscle or erythrocyte. The erythrocyte floats in a medium, the plasma, whose chemical composition is much the same in all species of mammals. The composition of the cell itself, however, varies in a very interesting way from species to species. This is indicated by the analyses of Abderhalden [1] which show that while the corpuscles of all of the forms examined contain less sodium than the surrounding plasma—those of some species indeed containing almost none—the concentration of potassium on the other hand is nearly always much greater in the cells than in the surrounding medium, that in the rabbit being approximately twenty times as great. There seems to be the best of evidence, both osmotic and electrical, that these differences are not due to any chemical combination of the potassium and sodium with indiffusible substances; both appear to exist as simple ions whose diffusion is prevented in some other way. Under certain conditions, as Kerr [2] has recently shown, sodium may diffuse inward and potassium outward in a way that would, if long enough continued, everywhere equalize their concentrations. Usually, however, this process cannot occur until the death of the cell.

Now the mere prevention of diffusion is not in itself remarkable; what is really surprising is that such differences can be set up in the first place. For potassium to collect in a cell, as it does here and in several other known instances, it

[1] Abderhalden, E. *Zeitschr. f. physiol. Chem.* 25, 65–115, 1898.

[2] Kerr, S. E. *Journ. Biol. Chem.* 85, 47–64, 1929.

is necessary for it to diffuse up-hill, as it were, against a concentration gradient. This seems to be opposed to the physical laws of diffusion.

The collection of potassium against a very high concentration gradient sometimes occurs throughout the entire life of a growing cell, as in the case of the marine alga, *Valonia*.[3] This plant consists of a single huge cell which may contain within it several cc. of cell sap. The concentration of potassium in the sap is usually approximately 40 times as great as in the surrounding sea water and it can be shown that it exists in a freely diffusible form. Nevertheless, not only does the potassium not leave the cell but it continues to enter it as long as growth occurs. Only with the death or injury of the organism does diffusion take place in what appears to be the proper direction.

How are such apparent contradictions of physical laws to be explained? The possibility has at least been considered by persons of the highest scientific standing that the Second Law of Thermodynamics, in its usual form at least, may not apply to living cells but that such cells may be able in some way to exert an element of choice in the manner postulated for Maxwell's demon. As a matter of fact, we have no conclusive evidence either for or against such a view. For all we know living cells may fail to obey the law in question. But we are not forced to make such an assumption in the case of the accumulation of potassium or in that of several other apparently puzzling phenomena which I shall mention this evening. It turns out that the properties of certain fairly simple membranes are sufficient to account for these and most of the other known facts concerning the diffusion of dissolved substances into living cells. Let us therefore turn our attention to such membranes.

The view that the intake of substances into the cell is regulated by a plasma membrane of some sort at its surface rather than by the properties of the protoplasm of the cell as a whole is a fairly old one which from time to time down

[3] Osterhout, W. J. V. *Journ. Gen. Physiol.* 5, 225–230, 1922.

to the present day has been disputed. It may be well, therefore, to mention a most convincing piece of evidence, in furnishing which I had a very modest share, in support of the membrane theory.

Some years ago I observed [4] that if a starfish egg, stained lightly with neutral red to serve as an indicator of its internal reaction, be removed from alkaline sea water of pH 8.1 and be placed in an acid solution of ammonium chloride of perhaps pH 6.0, the internal reaction instead of changing in the same direction as that of the surrounding solution changes in the opposite direction, the neutral red becoming distinctly orange in color. At first sight this is rather a surprising result, though it is, as a matter of fact, very easily explained. If now the cell be removed to a slightly alkaline bicarbonate buffer system of, let us say, pH 7.4, the internal reaction again changes, but, as before, in the opposite direction from that of the surrounding medium, the neutral red assuming the bright pink color characteristic of acidity. If, on the other hand, instead of living cells dead cells be used, the changes in reaction are in each case those which might more naturally have been expected, namely, in the same direction as that of the surrounding medium.

At first sight it might seem that we have here an example of the ability of a living cell to reverse the normal direction of diffusion processes. As a matter of fact, the explanation turns out to be a far simpler one. In the case of the ammonium chloride solution there are present in addition to the dissociated salt certain amounts of hydrochloric acid and ammonia resulting from hydrolysis of the salt. The hydrochloric acid is dissociated and gives the solution its acid reaction; the ammonia is largely undissociated. Of the various substances present, only the undissociated ammonia can penetrate the living cell at all readily; it is this that causes a change in the alkaline direction. Similarly, in the alkaline bicarbonate mixture there are present considerable amounts of free CO_2 which alone can enter the cell. It is this which

[4] Jacobs, M. H. *Journ. Gen. Physiol.* 5, 181–188, 1922.

gives rise to the increased intracellular acidity. There is therefore no contradiction here of the law of diffusion in the living cell; it is merely a question of the prevention by it of the diffusion of some things which are able to enter dead cells. Exactly the same results can be produced by an artificial model of the cell in which the substances which enter must first pass through a layer of xylene.[4] Since ammonia and CO_2 dissolve in the xylene and the other substances do not, we are able with an extremely simple non-living system to secure essentially the same results as with living cells.

The question now arises: Are the substances which in the cell perhaps correspond to the xylene in the model distributed throughout the entire protoplasm or are they localized at its surface in a membrane of some sort? In talking the question over with Professor Robert Chambers, whose microdissection and microinjection methods have done so much to clear up disputed questions of cell physiology, it seemed that a direct answer could be furnished by the use of micrurgical technique. It was my good fortune to be permitted to observe through a demonstration ocular attached to Dr. Chambers' microscope the experiments by means of which he settled this question most conclusively and dramatically.

The experiments were as follows.[5] Eggs were first stained and placed for a few moments in the ammonium chloride mixture until the color of the intracellular indicator had changed visibly in the alkaline direction. Some of the same ammonium chloride solution was then injected into an individual egg. Immediately the indicator assumed its acid color at the point of injection and, what is more important, there was a rapid spread of the acid condition in all directions from the point of injection which did not cease until the boundary of the egg had been reached. The last region of the egg in which the change from alkalinity to acidity occurred was therefore that which from the beginning of the experiment had been almost in contact with the solution in question. Experiments with the carbon-dioxide-bicarbonate buffer system

[4] Chambers, R. *Journ. Gen. Physiol.* 5, 189–193, 1922.

gave results which were entirely the same in principle except that the conditions of acidity and alkalinity were the reverse of those just described.

From these and several other lines of evidence, it seems that the existence of cellular membranes capable of regulating the intake of diffusing substances can be considered to be well established. The next matter of importance is to gain further knowledge of the properties of such membranes. My own work in recent years has been chiefly in this field. Unfortunately, microscopic methods are here of little assistance. The membranes in question are frequently so delicate that they cannot with certainty be seen. On the other hand, some visible membranes at cell surfaces may turn out to have little physiological importance in regulating the intake of substances by the cell.

An interesting case of the latter sort was discovered and investigated a few years ago by one of my students, Mr. E. J. Nadler.[6] He worked with a unicellular organism, *Blepharisma*, which has a peculiar pinkish color localized in an outer layer or pellicle. The animal can without any apparent injury be induced to shed this pellicle, which is normally an apparently inseparable part of its anatomy, by treatment with certain concentrations of strychnine and a number of other substances. After the loss of the pellicle a new one is ultimately regenerated, but, in the meantime, the permeability of the animal to dissolved substances does not appear to have been noticeably changed. In other words, the outermost visible membrane is here not the physiological plasma membrane which regulates the intake of substances from the surroundings of the animal. The membrane of real importance must lie deeper.

The most successful method for investigating the properties of cell membranes is the physiological method of comparing the rates of entrance of a variety of different chemical substances into cells and then determining what physical and chemical characteristics are shared by the substances which

[6] Nadler, E. J. *Biol. Bull.* 56, 327–330, 1929.

enter rapidly, by those which enter at an intermediate rate, and by those which enter with difficulty or not at all. From data of this sort more or less plausible deductions may be made concerning the nature of the plasma membrane. The first extensive studies of this sort were made by Overton [7] about 35 years ago on plant cells. Overton's main results as far as they went have been confirmed for many other types of material, but in recent years a number of new facts of considerable interest have been added to them.

My own work has been chiefly on the mammalian erythrocyte which in many respects is a very convenient type of cell to use for studies on cell permeability. One of its greatest advantages is that the results obtained with it are frequently visible to the naked eye. When placed in water, or a sufficiently hypotonic solution of any dissolved substance, the erythrocytes in a drop of blood swell and give up their hemoglobin to the surrounding medium and the originally turbid suspension becomes transparent. If they are placed in a solution of a penetrating substance not sufficiently dilute in itself to cause hemolysis, the same changes will occur at a rate which depends on the rate of penetration of the dissolved substance. The times of hemolysis in a series of solutions of different substances of the same concentration may, with certain limitations which need not here be discussed, be used as a measure of their rates of penetration.

Using this simple method, let us compare for the corpuscles of the ox the rates of penetration of three chemically related substances,—glycerol, monoacetin and diacetin, all at a concentration osmotically equivalent to that of blood. It will be observed that of the three substances diacetin enters most rapidly, producing hemolysis in 5 or 6 seconds. Monoacetin requires perhaps 40 or 45 seconds while the corresponding time for glycerol is about 45 minutes. It is evident that the order of penetration is exactly the reverse of that of the molecular weights of the substances and of their rates of diffusion in water. It is also the reverse of that with which

[7] Overton, E. *Vierteljahrschr. d. Naturforsch. Ges. Zurich* 40, 158–201, 1895.

they diffuse through certain artificial water soaked membranes studied by Collander.[8] It is, however, the same as that of their solubility in many fatty materials and fat solvents such as ether or xylene. The same result may be obtained with other similar groups of compounds and is a good illustration of the generalization of Overton that cell permeability and lipoid solubility frequently go hand in hand.

That the living cell behaves as though it were surrounded by a lipoid film of some sort is also indicated by the peculiar changes of reaction, already described, which occur in solutions of ammonium salts and in bicarbonate buffer systems and which can be imitated by an artificial cell in which a layer of xylene is the selective agent. On the other hand, there is much to show that the plasma membrane is more than a mere film of lipoids. In the first place, water, one of the most freely penetrating of all substances, is insoluble or only slightly soluble in most of the lipoids and lipoid solvents. In the second place, there are other substances of low molecular weight which in spite of very feeble lipoid solubility enter cells with ease. In these cases the size of the molecule appears to be the determining factor.

The importance of molecular size in cell penetration may be illustrated by another simple experiment, this time with rat blood, with the three closely related compounds, ethylene glycol, glycerol, and erythritol. None of these compounds is of the type that is at all freely lipoid-soluble; they do, however, form an excellently graded series with respect to molecular weight and molecular size. The reason for choosing rat blood rather than ox blood is that with the latter penetration is too slow for an experiment of this sort; it is also of interest to be able to make a comparison of the penetration rate of glycerol with the bloods of two different species.

The result of this experiment is to show that with ethylene glycol a sufficient amount enters the cells to produce hemolysis in perhaps 6 or 7 seconds; with glycerol the time is possibly 20 seconds, while with erythritol several hours are usually

[8] Collander, R. *Societ. Scient. Fennica. Commentat. Biol.* II, 6, 1–48, 1926.

required. For the ox the times are of the order of magnitude of 30 seconds for ethylene glycol, 45 minutes for glycerol, and an indefinitely long time for erythritol. Since it seems likely in cases such as these and in a number of others which might be mentioned that where lipoid solubility is low the factor of most importance is the size of the molecule; we may summarize our knowledge of the permeability of the erythrocyte very briefly as follows: *If molecules are sufficiently small they will enter the cell regardless of their lipoid solubility; if they are sufficiently lipoid soluble they will enter the cell regardless of their size.*

Two questions arise in connection with the factor of molecular size. The first is: What property of the membrane determines that small molecules shall be admitted and large ones kept out; and the second is how small must a molecule be in order to be sure of securing admittance? With regard to the first question, the most plausible explanation is that the membrane has a porous structure of some sort, the size of the pores determining the size of the molecules that can enter. That this is not an unreasonable hypothesis is indicated by the fact that we know of several artificial membranes of exactly this type—for example, the ordinary water-soaked collodion membrane so much used in dialysis experiments.

The second question is more difficult to answer since a great deal depends on the material used. Even for a single type of cell, such as the erythrocyte, there are pronounced specific differences. I have recently been interested in studying such specific differences in the case of a number of selected compounds of which I shall here refer to ethylene glycol, glycerol and erythritol. It turns out that the differences between the erythrocytes in this purely physiological character are far more striking than any that can be seen with the microscope. Morphologically, indeed, the erythrocytes of different mammals are almost indistinguishable, the only visible differences in most cases being those of size; and these are frequently difficult or impossible to detect. The differences in permeability, however, prove to be so great and so

constant that by means of them alone it is possible to distinguish any one of the 10 or 12 kinds of blood which have been studied from all of the others. Consider, for example, the difference already mentioned between the blood of the ox and that of the rat with respect to permeability to glycerol, the time for hemolysis in the former, under exactly the same conditions, being possibly 150 times as great as in the latter.

TABLE 1

	S	E	G	(E-W)/W	(G-W)/W	Erythritol
Rat	4.2	6.6	18.8	0.5	3.5	½ to several hours
Mouse	3.0	8.6	38.8	1.9	12.9	Less than 5 minutes
Rabbit	3.0	11.3	79.9	2.2	21.8	6–18 hours
Guinea pig	5.0	15.7	196.0	2.1	38.2	6–18 hours
Man	8.35	12.6	42.7	0.5	5.1	6–18 hours
Dog	6.1	28.6	1548.	3.7	253.	6–18 hours
Cat	2.65	18.3	1222.	5.9	459.	6–18 hours
Pig	3.0	16.7	1024.	4.6	340.	6–18 hours
Ox	3.8	35.1	2325.	8.3	612.	More than 24 hours
Sheep	1.9	24.1	1623.	11.7	850.	More than 24 hours

Further facts of the same sort are presented in Table 1. In it the columns, S, E and G show the times required for hemolysis in a strongly hypotonic salt solution and in the same solution containing 0.3 M ethylene glycol and 0.3 M glycerol, respectively. In the following two columns an attempt has been made to apply a rough correction for certain other properties of the corpuscles in order to obtain a fairer comparison than would be possible from the time of hemolysis alone.

It is interesting in connection with the figures for glycerol to note certain evidences of zoölogical relationship. For example, all the rodents studied, namely, the rat, mouse, rabbit and guinea pig, are similar in their very great permeability. The closely related sheep and ox, on the other hand, fall together at the other end of the series. It will be of interest when it is possible to obtain blood from one of the

higher apes to determine whether it will show a similarity in this character to that of man. I predict that it will.

With regard to erythritol, its penetration is so slow in nearly all cases that secondary changes in the erythrocytes probably have time to occur before hemolysis takes place. The exact times of hemolysis, therefore, do not mean a great deal. In the case of the mouse and the rat, however, penetration is fairly rapid—indeed, for the mouse it is more rapid than that of glycerol with most of the other species studied. By this character alone the blood of the mouse can immediately be distinguished from that of any of the other species listed.

How is it possible for the corpuscles of the mouse to have a higher degree of permeability than those of the rat to the large erythritol molecule without at the same time showing a higher degree of permeability to the smaller glycerol molecule? Are such facts compatible with a pore theory? The answer to this question depends upon one consideration: are the pores all of the same size? If so, the facts are incompatible with the theory; if, however, the pores are of different sizes the theory need not necessarily be abandoned. For example, if the erythrocyte of the mouse had a number of large pores and not very many small ones, while that of the rat had fewer large pores but many more somewhat smaller ones, the known facts concerning these two substances could be accounted for.

This is, of course, purely a hypothetical picture. It is not even certain that these substances enter primarily by means of pores, though there is considerable evidence that this is the case, which must be omitted for lack of time. In any event, it is evident that studies of this sort, which are still in their infancy, suggest a method for throwing light upon the interesting question of the finer ultra-microscopic structure of the plasma membrane.

The behavior of the erythrocyte so far described would seem to demand a combination of penetration by solution and penetration by way of pores. It is not impossible to construct artificially a model showing penetration by both of these methods. For example, one of my students, Miss

Buisset, has obtained evidence, as yet unpublished, that the ammonium salts of the lower saturated fatty acids penetrate moist collodion membranes by way of pores and dried collodion membranes by solution in the substance of the membrane. It is easy to convert a dried collodion membrane into one of the moist type by soaking it in alcohol of some desired strength and then transferring it to water. By this treatment pores appear to be formed in it. If now there were merely placed here and there on a dried collodion membrane drops of alcohol and the membrane were then soaked in water, it would now combine regions permitting one type of permeability with those permitting the other type. The cell membrane is undoubtedly more complex than such a crude model would be—for one thing, it seems to contain both proteins and lipoids—but at any rate there are no theoretical difficulties in postulating a combination of the two types of permeability in a single membrane.

We come now to a very important set of facts having to do with the behavior of electrolytes in the presence of cell membranes. Incidentally, these facts, as far as the erythrocyte is concerned, greatly strengthen the pore theory.

For many years it has been known that anions such as Cl', HCO_3', NO_3', etc. readily enter and leave erythrocytes, while cations such as $Na^{\cdot}$, $K^{\cdot}$, and $Ca^{\cdot\cdot}$ do not. Since it is essential that an electrical balance of anions and cations shall be preserved inside and outside of the cell and since it is impossible for cations to accompany anions in their movements across the membrane, the only possibility is for one kind of anion from inside to be exchanged for another kind of anion from outside. Thus, in the normal respiratory cycle in the human body, there is known to be a shift of Cl' in exchange for HCO_3', the Cl' ions entering the erythrocytes in the tissues and leaving them in the lungs. This shift is governed by the Donnan law for such systems, according to which

$$[Cl]_{\text{inside}} \times [HCO_3]_{\text{outside}} = [Cl]_{\text{outside}} \times [HCO_3]_{\text{inside}}.$$

This shift of anions has certain very important consequences

in connection with the preservation of a constant blood reaction.

My own contribution to this part of the field has been a very modest one, being confined merely to the removal of the one apparent exception to the statement that the membrane of the erythrocytes is permeable to anions and impermeable to cations. It had been known for many years that the erythrocyte, unlike most other cells, is permeable to highly dissociated ammonium salts such as the chloride and the nitrate and it was very generally believed that in this case the ammonium ion was exceptional in being able to enter the cell. I pointed out [9] that it is not only not at all necessary to postulate a permeability of the cell to this particular cation but that the known facts can be much better explained by a scheme involving the entrance into the cell of undissociated ammonia followed by an exchange of OH' ions from inside the cell for Cl' ions, etc. from outside.

We have, therefore, in the erythrocyte a cell which under normal conditions appears to be permeable to anions and without exception impermeable to cations. How is this differential permeability to be explained? The solubility theory does not seem plausible. We know of no solvent which dissolves anions and not cations. On the other hand, it has been shown by Michaelis and his co-workers that certain artifical membranes, such as those of dried collodion, etc. are permeable to one kind of ions, cations, and not at all, or to a less extent, permeable to anions according to the character of the pores.

It is true that most artificial membranes are permeable to cations rather than to anions, while the erythrocyte is permeable to anions rather than to cations, but this distinction is not an important one. In the first place, Höber and Hoffmann [10] have recently succeeded, by adding certain dyes to their collodion, in making membranes which are permeable

[9] Jacobs, M. H. *The Harvey Lectures.* Series 22, 146–164, 1927.

[10] Höber, R. and Hoffmann, F. *Pflüger's Arch. f. d. ges. Physiol.* 220, 558–564, 1928.

to cations rather than to anions or even mosaics of the two sorts. In the second place, Mond [11] has shown that at sufficiently alkaline reactions the permeability of the erythrocyte to the two kinds of ions may be reversed. We may say, therefore, that the permeability of this cell to electrolytes is in very satisfactory agreement with the pore theory.

The erythrocyte is a cell which is very highly specialized in one particular direction, its ready permeability to anions being associated with a special physiological function and apparently not being shared by most other cells. In fact, such evidence as there is seems to indicate that most cells, like the majority of artificial membranes, are more permeable to cations than to anions.

The evidence for this statement is chiefly of an electrical nature and is based on a method used extensively by Michaelis [12] and his co-workers in their studies on collodion membranes. It is applicable even when the permeability is too slight to be studied by chemical means. Its principle is, briefly, as follows. If a membrane be permeable to cations and not to anions, then even though the cations are unable to leave the membrane because of the attractive forces of the anions with which they are paired, their tendency to do so will give an electrical potential difference between the two sides of the membrane, the more dilute solution being positive to the more concentrated one. If, on the other hand, the membrane be permeable to anions and not to cations, then the more dilute solution will be electrically negative. Furthermore, the valence and the mobility of the ions to which a membrane is permeable have a great effect upon the observed potential differences; the valence and mobility of ions to which it is impermeable have little effect. By measurements of potential differences with variations in the electrolyte used and in its concentration on the two sides of the membrane, the properties of the membrane may be very satisfactorily investigated.

[11] Mond, R. *Pflüger's Arch. f. d. ges. Physiol.* 217, 618–630, 1927.

[12] Michaelis, L. *Journ. Gen. Physiol.* 8, 33–59, 1925.

Studies of this sort with sheets of cells such as the skin of the frog and the wall of the stomach have been made by several workers and as far as they go they seem to indicate a greater permeability to cations than to anions. An interesting study of this sort was made a few years ago on the egg of the fish, *Fundulus*, by one of my students, Miss Margaret Sumwalt.[13] This study had its origin in an attempt to furnish an explanation of certain results obtained with this material by Loeb and Cattell[14] in 1916 and not at that time satisfactorily explained. The observation was that if eggs of *Fundulus* containing developing embryos are placed in solutions of potassium chloride, potassium enters them and paralyzes the hearts of the embryos. If they are then removed to distilled water or to solutions of non-electrolytes, the potassium fails to escape; if, however, they are placed in electrolyte solutions, the potassium promptly diffuses outward and the heart resumes its beat.

After the appearance a few years ago of the work of Michaelis, it seemed to us that if the membrane of the *Fundulus* egg should prove to be permeable to cations and not to anions, the observed facts could readily be explained. Under these conditions, potassium would leave the cell only if it could be exchanged for another cation; it could not otherwise, in the ionic form, diffuse away from its partner, chlorine. By a rather difficult technique involving the introduction of microelectrodes into single eggs of *Fundulus* and therefore requiring much care and patience Miss Sumealt was able to show in a very satisfactory way that the permeability of the outer membrane of this egg is, in fact, that demanded by the theory.

If now cells, in general, because of a porous membrane of some sort are more permeable to cations than to anions, then it becomes possible to explain in a very simple way the otherwise puzzling accumulation of potassium already mentioned, which at first sight appears to defy the laws of diffusion.

[13] Sumwalt, M. *Biol. Bull.* 56, 193–214, 1929.

[14] Loeb, J. and Cattell, McK. *Journ. Biol. Chem.* 23, 41–66, 1915.

The credit for this explanation should, I think, go to a German worker, Netter,[15] though essentially the same idea occurred to a number of persons at about the same time. Its principle can be made clear by a simple analogy.

It is, of course, just as fundamental a law of nature for water to flow down hill as for dissolved substances to pass from a region higher to one of lower concentration. But it is by no means impossible to make water run up hill. It is done every day in thousands of places in the world by hydraulic rams. No laws of nature are contradicted by this arrangement; a small quantity of water is merely forced up hill at the expense of a considerably greater amount which flows down hill. The total flow is always in the right direction.

Similarly, in diffusion processes it is theoretically possible to use a downward flow, so to speak, of one ion to produce an upward flow of another. Now, in at least most living cells there is maintained a constant gradient from within outward of a certain kind of ion, the hydrogen ion. These ions are continually appearing in the cell from such products of metabolism as carbonic and lactic acids, etc., and even though the acids tend to diffuse away they are constantly being renewed.

The difference in hydrogen ion concentration between the cell and its surroundings may be considerable; in the sap of *Valonia* the internal concentration is maintained at a value several hundred times as great as that in the surrounding alkaline sea water. In the developing erythrocyte (which, incidentally, in its early stages probably resembles ordinary cells in being more permeable to cations than to anions) the difference is not so great but must still be considerable. This steadily maintained gradient of hydrogen ions can be likened to a constant source of water power, which in its descent lifts other material to a higher level.

As to the mechanism of the process, if the cell membrane be permeable to cations and not to anions it is not only possible but necessary for an exchange of hydrogen ions for other ions to occur in the manner already described for anions in the

[15] Netter, H. *Pflüger's Arch. f. d. ges. Physiol.* 220, 107–123, 1928.

erythrocyte. For a given ions, *e.g.*, K, the equilibrium condition which tends to be approached, though it may never be reached, is:

$$[H]_{inside} \times [K]_{outside} = [H]_{outside} \times [K]_{inside}.$$

By a downhill diffusion of hydrogen ions, therefore, an uphill diffusion of other ions may be brought about without the contradiction of any physical laws.

The only remaining question is: Why do potassium ions rather than sodium ions accumulate in the cell? The answer is very simple. Because of its lesser degree of hydration, the mobility of the potassium ion is considerably greater than that of the sodium ion, and Michaelis has found that these differences are enormously exaggerated by certain membranes. Pores which admit potassium and not sodium, or potassium much more readily than sodium are not difficult to imagine. One of my students, Miss Howard,[16] has recently obtained some evidence of the existence of the latter condition in the egg of *Arbacia*. By means of minor variations of this sort in the degrees of permeability of the respective membranes to sodium and potassium the various specific differences mentioned above could readily be accounted for. In short, it appears entirely possible that the accumulation of potassium in varying degrees in different living cells which at first sight seems to contradict the laws of diffusion may in reality be in entire agreement with them.

It is, of course, one thing to give a possible explanation of a given phenomenon and another to prove that it is the correct one. But in the investigation of physiological problems it is at least an enormous help to the morale of the workers to feel that the problems with which he is dealing are not hopeless ones. If it could be conclusively shown that the intake of substances by living cells is not strictly governed by the Second Law of Thermodynamics—which for all we know may be the case—it would undoubtedly be a most interesting discovery. But if such a discovery were ever made it is safe to say that it

[16] Howard, E. *Biol. Bull.* 60, 132–151, 1931.

would be very promptly put an end to most of the work in the field which I have been discussing this evening. For the present, therefore, in the absence of any real evidence of the inapplicability of accepted physical laws to physiological systems, let us continue to enjoy to the full the greatest joy of the scientist, namely, that of attacking problems which we know are enormously difficult but which we believe are not insoluble.

THE ELEPHANT ENAMEL METHOD OF MEASURING PLEISTOCENE TIME. ALSO STAGES IN THE SUCCESSION OF FOSSIL MAN AND STONE AGE INDUSTRIES

By HENRY FAIRFIELD OSBORN and EDWIN H. COLBERT

(*Read April 25, 1931*)

DURING the last three years *Pithecanthropus*, the Trinil man of Java, and *Eoanthropus*, the Piltdown man of Sussex, have been re-dated by the combined researches of Professor W. O. Dietrich of Berlin, Professor Wilhelm Freudenberg of Heidelberg and Professor Osborn of the American Museum. The re-dating method consists of much closer study than has been given before to the posterior grinding teeth of the stegodonts and elephants found associated with the fossil remains of these two classic stages in human pre-history. In the case of *Pithecanthropus* Dietrich and Osborn observed that the grinders of both the associated Proboscideans were not of closing Tertiary age, as Dr. Eugene Dubois the discoverer of *Pithecanthropus* supposed, but of middle Pleistocene age, because far more progressive than any upper Tertiary, or Pliocene, Proboscidean. Osborn secured from Dr. Dubois a fine specimen of the elephant grinder which proved, by comparison with the middle Pleistocene elephant grinder (*Palæoloxodon namadicus*) of India, to be of middle Pleistocene age. It appears from this astonishing result that *Pithecanthropus* is not an ancestral but a primitive form surviving in the isolated forests of northern Java.

Meanwhile Freudenberg pointed out that the Proboscidean tooth fragments found in the Piltdown gravels with *Eoanthropus* were apparently similar to corresponding parts in the upper Pliocene elephant of India and southern France known as *Archidiskodon planifrons*—the flat-headed archaic ancestor of one of the great elephantine sub-families. Guided by this

observation Osborn has devoted two years of research to the upper Tertiary elephantine grinders of India and of southern France. These demonstrate conclusively that *Eoanthropus* was a companion and probably a hunter of the primitive flat-fronted elephant, consequently of late Tertiary age.

The success of this combined research led Osborn to the very close examination and comparison of all the posterior grinding elephantine teeth which have thus far been discovered, described and figured in the museums of India, Europe and North America. This comparison leads to the unexpected result that the enamel platings of the elephantine grinders of more than one line of descent may be used as a new method not only of dating various pre-historic stages of man but of estimating the duration of the sub-divisions of the age of man which have hitherto been dated chiefly by geologists calculating the length of the four glacial and three interglacial epochs.

Employing the Greek term "ganos" signifying enamel, this new method may be called *ganometric*. The combined enamel foldings of the superior grinding teeth, when drawn out of their closely plicated arrangement for the finer comminution of the herbage on which these animals subsist, steadily increase in length from upper Pliocene time when *Eoanthropus* lived to closing Pleistocene time when the late Cro-Magnon man lived. The foldings of the molar teeth of the Proboscideans contemporaneous with *Eoanthropus* measure a little more than a meter or about a yard in length while the enamel foldings of those contemporary with Cro-Magnon man attain over six meters in length. Since from other very strong evidence these enamel layers are known to be very uniform in evolution it is obvious that the six meter length arose centimeter by centimeter during the estimated 1,000,000 years of the age of man. Thus as this ganometric research proceeds we shall probably be able to ascertain just how long it took to produce a centimeter of enamel length. These finer measurements and calculations remain for future very precise research and intensive re-examination of all the elephant grinding teeth in the various great museums of the world.

One of the most important methods of checking off the final results and estimates will be the comparison of the four entirely independent lines of elephantine evolution which are known to have taken place side by side beginning with upper Pliocene and Pleistocene time. In certain lines like the flat-fronted elephant *Archidiskodon planifrons* to the Imperial Mammoth *Archidiskodon imperator*, the enamel layer growth was relatively rapid, whereas in other lines such as the straight-tusked elephants first found with the Heidelberg man (*Palæanthropus heidelbergensis*) and finally found with the Krapina stage of the Neanderthals (*Palæanthropus krapinensis*) the enamel growth was relatively slow.

A preliminary summary of the enamel evolution of the third superior molars in six distinct phyla is roughly estimated as follows:

STEGODON:

Stegodon airawana, Middle Pleistocene, M^3, 510 mm.
Stegodon bombifrons, Lower Pliocene, M^3, 410 mm.

In all the Stegodons the posterior grinders evolve very slowly and are in wide contrast to the extremely rapid evolution of the enamel in the contemporary Proboscideans.

ARCHIDISKODON:

Archidiskodon imperator, Lower Pleistocene of Nebraska, (maximum) 7200e mm.
Archidiskodon planifrons, Upper Pliocene of India, (minimum) 825 mm.

This is an extreme case of the rapid growth of enamel over a period of time estimated by geologists at 350,000 years. A progressive *Archidiskodon planifrons* M_3 (2250 mm.) is contemporary with the Piltdown man of Sussex.

PALÆOLOXODON:

Palæoloxodon antiquus italicus, Upper Pleistocene of Italy, 6700 mm.
Palæoloxodon antiquus germanicus, Upper Pleistocene of Germany, 5200 mm.

Palæoloxodon antiquus typicus, Lower Pleistocene of England, 5350 mm.

This Palæoloxodon or 'straight-tusked' phylum is remarkably complete and is represented by an enormous number of grinding teeth in Europe which may be very accurately measured. It is extremely important because the *Palæoloxodon antiquus typicus* (5350 mm.) is contemporary with Heidelberg man while the *Palæoloxodon antiquus italicus* (6700 mm.) is contemporary with the Neanderthal man of Krapina.

PARELEPHAS:

Parelephas progressus, final, Pleistocene American stage, 9700 mm.

Parelephas columbi, primitive Upper Pleistocene of America, 6210 mm.

CONCLUSIONS

1. During the upper Pliocene and first half of the Pleistocene there was a slow increase in the molar enamel lengths of the Stegodons, compared with the rapid increase in the elephants.

2. In the elephants the enamel increase was different in each separate phylogenetic line, very rapid in *Archidiskodon*, very slow in *Palæoloxodon*, probably still slower in the African elephant, *Loxodonta*.

3. In the long *Parelephas* phylum extending from the lower Pleistocene of England to the upper Pleistocene of North America the final stage is as follows:

Total enamel length of LM^3, 9700 mm.
Total enamel length of LM_3, 6600 mm.

4. In *Parelephas* the enamel length is greater in the upper molars (9730 mm.) than in the lower (6600e mm.), but this ratio varies within different phyla.

5. In species like *Archidiskodon planifrons*, which embraces a large number of ascending mutations found in the Siwaliks of India and in southern France, the enamel length ranges from a minimum of 825–1060 mm. to a maximum of 2500e mm., that is, it nearly doubles.

6. (Note by Colbert.) The measurements thus far assembled are all rough and preliminary; out of the thirty-six teeth studied, only two were unworn; consequently there are large estimated factors in most of the measurements.

7. (Note by Colbert.) The enamel length did not increase at a constant rate. It was slow during the lower and middle portions of the Pleistocene; it was greatly accelerated during the upper Pleistocene.

8. Osborn believes that when it is possible to make more refined and accurate measurements the enamel length will be found to be very rapid during the highly favorable climates of upper Pliocene and lower Pleistocene time as in *Archidiskodon*, relatively constant and steady during the Pleistocene up to the close of third interglacial time when all the elephants in Europe and America disappear except the woolly mammoth.

9. Finally the growth figures in the ancestors of the mammoth, which are now known from the lower to the upper Pleistocene in Europe, exhibit a very steady addition of ridge plates from sixteen in the lower Pleistocene (*Mammonteus astensis*) to twenty-seven in the upper Pleistocene (*Mammonteus compressus*). The total enamel length in the latter (7300e mm.) is about equal to that of *Archidiskodon imperator* of the lower Pleistocene (7200 mm.). It is inferior to that of *Parelephas progressus* of the upper Pleistocene (9700 mm.). Relatively however to the greatly dwarfed stature of the woolly mammoth (nine feet) as compared with the tremendous stature of the Imperial Mammoth, *Archidiskodon imperator* (thirteen feet), the enamel length of *Mammonteus compressus* greatly exceeds that of the giant *Archidiskodon imperator*.

These preliminary results are from approximate measurements, consequently it is not as yet practical to date the Stone Age men by them. They will be presented in much more mature and thoroughly considered form before the Centenary Meeting of the British Association to be held in London during the month of September, 1931.

SOME ECONOMIC ASPECTS OF ROME'S EARLY LAW

By TENNEY FRANK

(Read April 24, 1931)

Rome's greatest gift to the world was her code, the product of a thousand years of legislating. Historians, who are as much interested in causative factors as in the final product, have long attempted to trace the development of early Roman law; but that is not an easy task, because the information is so fragmentary. The legal fragments that have survived from the five centuries of the republic—the formative period—can readily be printed on one hundred standard pages.

Editors of these fragments do not always agree as to what to include. The Roman authors who cited lines from the Twelve Tables, for instance, sometimes abbreviated their quotations, sometimes filled in explicative phrases out of more recent commentaries. The editors have had to create an apriori conception of what early Roman law was likely to be, and this conception has usually been deduced from a reading of standard histories of Rome, from a survey of comparative jurisprudence, and from inferences based upon classic law.

In forming their conceptions in this manner, scholars have, it seems to me, admitted some errors. In the first place, they have clung too conservatively to obsolete Roman histories. Few of them have attempted to follow the archæological discoveries made at Rome during the last three decades, discoveries that have quite revolutionized our ideas of what Rome was like during the century before the Twelve Tables were posted. The modern editor who follows Mommsen in supposing that fifth century Rome was a very primitive farm-community that had recently discovered the use of private property, and that had had no experience in commerce,

is apt to exclude from his edition all references to contracts of a developed form. We now happen to know that before the beginning of the republic Rome had a large seaborne commerce, a vigorous industry, and a large number of splendid buildings very charmingly decorated by Greek and Etruscan artists. That all implies rather advanced business methods and forms.

Because of their misconception of Rome's importance the editors have also misapplied the deductions that they have drawn from comparative jurisprudence, since they have compared early Rome with peoples of a lower scale of cultural experience than was justified. The Twelve Tables reveal some very primitive customs to be sure, but these customs usually have to do with religion, which is apt to be very conservative. The legal thought connected with inheritance, family, and trade is far more advanced. It will no longer do to compare the Greeks and Romans of three thousand years ago with fossilized and retrograde races. The mere etymology of the word *venum*—which occurs with the same meaning in Greek and Sanskrit—proves that trade was rife thousands of years before Rome, when the various peoples of the Indo-European stock still lived together. The exquisite language of Homer, still the despair of translators, proves the vast age of preceding culture. There is, I dare say, a greater mileage of cultural progress from the yelp of a primitive savage to a canto of Homer, than there is from a paleolithic artifact to an airplane. European anthropology has been based upon an inadequate conception of human evolution, and has misled the scholar in his use of comparative jurisprudence.

The chief error involved in employing classical law as an aid in interpreting older customs derives from a natural desire to map out a consistent line of progress from the Twelve Tables to Justinian's code, and to disregard clauses that do not fit into the constructed diagram. Yet no law has ever developed in a straight line. We know, for instance, that in England testamentary law was more advanced in 300 A.D.—before the Romans left—than it was a thousand years later,

when feudal customs had interfered with individual property-holding. At Rome the growth of a strong aristocracy, after the first code was made, worked toward conservatism in the laws of property and testaments. The Voconian law, for instance, marks a retrogression rather than progress. We have no right to assume that in all respects the law of the Twelve Tables was more primitive than that of Cato's day, and the editor who deletes quoted clauses according to such an assumption is employing an unsafe method.

To indicate the direction that our revisions of old interpretations will have to take I shall consider a few clauses of the early law of contracts, of wills, and of marriage.

In the matter of sales our most definite statement in the Twelve Tables refers to *mancipium* (Table VI, 1, *Cum nexum faciet mancipiumque, uti lingua nuncupassit, ita jus esto;* "When a contract or transfer is to be made, what the tongue has pronounced so the law shall do"). The sale by *mancipio* was, of course, relatively infrequent because it was obligatory only in the case of *res mancipi:* land (with its servitudes), slaves and cattle. The transaction called for five witnesses and a weighing out of the price in bronze (or a symbolic act representing the actual payment). As land could not be held except by *cives*, or by the Latins who had access to the court, this ceremony, confined to the native population, could and did continue long in the rural districts.

The sentence is, of course, by no means explicit and it is one of those that receive their interpretation largely from the interpreter's conception of what Rome's economic and social conditions were like at the time. The main question connected with the passage is whether such transactions must be completed at once by a transfer of goods and price or whether more liberal forms as, for instance, of credit transactions are to be assumed in the phrase *uti lingua nuncupassit.* Now, in a passage of Justinian's *Institutes* (II, 1, 41), which unfortunately modern editors fell into the habit of placing in table VII, thus removing it from its proper place as a comment on the articles of the sixth table, we are explicitly told that the

Twelve Tables permitted the satisfaction of the vendor by pledge or guarantee in place of immediate payment. That would seem to be clear, but Karlowa, *Röm. Rechtsgeschichte* II, 612, set the habit of saying that this clause could not have been applicable at the time of the Twelve Tables to sales of *res mancipi* because Rome was then too primitive to use sales on credit, and Kübler's recent hand-book (p. 51) agrees with him. Girard, *Manuel*, pp. 290 ff., does indeed accept it for *res mancipi*, but on the erroneous supposition that coinage was already in vogue in the fifth century.

In denying the appositeness of the clause it is customary to insist upon a very narrow interpretation of the phrase *uti lingua nuncupassit* (VI, 1). One school holds that these words referred only to verbal utterances as contrasted with written agreements; another that, since Festus (L. 276) quotes the phrase in connection with *pecunia*, it was applied only to the precise sum named as the purchase price. Neither interpretation is in any way compelling. The emphasis may equally well have been upon the word *uti*, and the most obvious meaning of the clause is that the parties to the transaction were to be bound by the agreement as spoken and witnessed by the five bystanders, whether it be a cash or a credit transaction. So far as this passage is concerned it rather implies than denies sales on credit, and in view of the clause quoted from Justinian it must be so taken.

Another favorite argument against an early date for credit transactions is the *a priori* assumption that the ritual connected with sales of *res mancipi* was not liberalized until coinage came into vogue. But the formality of the *libripens* had certainly become a symbol connected with credit sales even before, since Varro quotes the phrase *rauduscula libram ferito* from *veteribus mancipiis*. The use of *aes rude* preceded coinage by many centuries, and there can be no doubt that the use of one piece of *aes rude* in the transaction is symbolic and implies that the price is not of necessity immediately paid. Why not accept all the fragments that we have and read them at face value? Why insist that the Roman sale was more primitive in form than the language implies?

Those who have worked over the archæological evidence of early Rome as it has come forth in recent excavations can have but little patience with the conservative interpretation of the Twelve Tables that assumes Rome to have been only a farm village. We now possess the remnants of at least fifteen well-decorated buildings of some magnificence erected at Rome before the time of the Decemviri, and much evidence of vigorous labor guilds and of foreign trade. Historians now generally agree that the first treaty with Carthage was made about sixty years before the code was posted. This document proves that during the Etruscan régime Roman merchants had traded freely in the Punic parts of Sicily and Libya while the Carthaginians had traded as freely at Rome. The Cassian treaty of the Latin league (dating from about forty years before the code) assured the continuance of *commercium* between Romans and Latins and laid down the interesting provision that disputes regarding private transactions must be settled within ten days in the court of the city where the transaction took place. This would seem to imply that trade might rest on something else than mere simultaneous *traditio* by way of simple barter. Furthermore, the clauses of the Twelve Tables that deal with the rate of interest, with *nexum* and with *depositum* all point to contracts quite beyond the sphere of mere and immediate exchange of goods.

In a word we must assume that at the close of the regal period Rome was a large and busy commercial city where the old and simple forms of barter based upon the instantaneous exchange of goods no longer sufficed, and that merchants trading at Rome introduced many of the liberal forms of contracts that were in vogue at other ports. To be sure, there may have been some retrogression toward old Latin practices before the Twelve Tables were written, as in fact there was thereafter. But there is no reason why all the clauses quoted from the Tables in connection with the laws of purchase and sale should not be accepted at face value. They seem, it is true, to be more liberal than some of the clauses of a later day, but there are adequate historical reasons for

assuming that trade retrograded rather than progressed during the decades that followed the decemviral legislation. In fact, after the Etruscan princes were expelled from Rome foreign commerce almost ceased for a time, and building operations dwindled to meager proportions.

My second illustration is taken from the fragments that pertain to wills and legacies. According to the old Latin custom, if a man died without making a will, his property was divided equally between those who became *sui juris* at his death, that is his widow and his children. There was no suggestion of primogeniture or of preferring sons to daughters. The most important phrases regarding inheritance that are quoted directly from the Twelve Tables are the following (Table V, 3–4):

Tabula V, 3 (according to the reading given in the two earliest quotations of it, *Auctor ad Herennium*, 1, 23, and Cic. *De Invent.* 2, 148): paterfamilias uti super familia pecuniave sua legaverit, ita jus esto. (As the father shall order concerning his estate so the law shall do.)

Ibid. 5: Si intestato moritur, cui suus heres nec escit, adgnatus proximus familiam habeto. (If he dies without will, and no heir of his exists, the nearest agnate shall have his estate.)

In the first of these quotations the wording is probably correct so far as it goes, though it probably omitted the clause *tutelave suae rei* which occurs in Ulpian fr. 11, 14, and in Paulus, Digest 26, 2, 1. The shorter forms of the clause, which omit *familia* and substitute *suae rei* (Gaius 2, 224; Pomp. Dig. 50, 16, 120) or *de sua re* (Nov. 22, 2 pr.), are late, and the substitution is clearly due to an attempt to modernize the clause after the word *familia* had become ambiguous. We know now that in early Latin the word *familia* meant the same thing as *res*, that is, the estate. This passage should therefore be quoted as it appears in the early republican citations, and not as it appears in the works of later writers who omit *familia* or substitute latter-day equivalents for it. Whatever is to be said of testamentary rights under the Twelve Tables, the law apparently made no distinction in this matter between real estate and movable property.

A more serious question arises as to whether the second clause cited above (Table V, 5: *cui suus heres nec escit*) implies that if the testator has a *suus heres* he can not deprive this heir of his portion even by will. Now legalists are wont, in discussing this question, to assume the existence of a primitive Rome which was very near to community or family ownership of land when these laws were written. Mommsen's history, for instance, has left us a legacy of statements like these: "It was not in the power of the father arbitrarily to deprive his children of their right of inheritance," and "Since the arable land among the Romans was long cultivated upon *a system of joint possession*, and was not distributed until a comparatively late age, the idea of property was primarily associated with . . . estate in slaves and cattle (*familia pecuniaque*)." These misstatements are, of course, partly based upon an etymology of *familia* that we now know to be incorrect. Furthermore, very few historians now believe that there was a "system of joint possession" in early Rome. Private property in land seems to have been known even to the far distant ancestors of the Romans who, several hundred years before, lived in the Terramara settlements of the Po Valley. We believe now that Roman landlords had for centuries bought and sold, accumulated and frittered away landed property. We must also bear in mind that during the Etruscan régime, at least, there had been not a little "capitalistic" exploitation of large estates as is shown by the traces of an extensive drainage system in various parts of Latium. All property had for a long time been fluid in the market and any interpretation of these clauses that is based upon the hypothesis that the Romans were just emerging from the economy of primitive village communities in 450 B.C. is apt to be erroneous.

Now Pomponius says: "Verbis legis XII tabularum his 'uti legassit suae rei, ita ius esto' latissima potestas tributa videtur *et heredis instituendi et legata et libertates dandi* tutelas quoque constituendi." (By the words of the Twelve Tables 'uti legassit, etc.,' it appears that the widest latitude was

given in instituting heirs, in bestowing legacies and manumissions, and in forming guardianships.) This statement is generally questioned, partly because of the misconceptions handed down by Mommsen's history and partly because later law is found to contain several restrictions on a man's power to bequeath his property. For instance, the *lex Voconia* was passed in the second century to prevent an estate from going out of a family through a female line, and later the *lex Falcidia* was passed to protect the putative rights of the natural heirs.

The truth of the matter is that the trend of Roman law in inheritance was actually away from the early testamentary freedom toward restriction, just as it was in medieval Europe under the feudal system. The trend was toward protecting estates and keeping them in the hands of those members of the family who had to bear the burden of civic obligations for the family, a trend that is natural in aristocratic society. In the early Republic, after a long period of commercial activity had removed property from communal restrictions, it was wholly reasonable that land, like other property, should have been at the complete disposal of the *pater familias* as it had been in Mesopotamia eighteen centuries before. And as *patria potestas* was still so strong that the *pater* had complete power over the life of his son, it is hardly conceivable that at this stage of property-rights he did not also have complete power over his property. If he could legally condemn his son to death, he doubtless could also disinherit him. I can therefore see no reason why we should question Pomponius—who had access to the complete text of the Twelve Tables—in his statement that the *pater* once had complete liberty as regards the making of wills and legacies. The customary neglect of this statement of Pomponius is due to a general misconception of the direction that the later law of property took during the development of aristocratic power and government.

Similarly there is another ceremony, that of *coemptio* in marriage, that seems to be misinterpreted, though in this case the cause of error was a misplaced anthropological theory.

It will be remembered that there were in old practice four different forms used in marriage: (1) *usus*, whereby free-marriage became a formal marriage with *manus* by cohabitation for a year, (2) *confarreatio*, a very formal marriage with religious sacrifices, for a long time employed by some of the patrician families, (3) *coemptio*, in which the formalities of *aes et libra*—ordinarily used in sales and testaments—were employed, and (4) *free marriage*, in which there need be no ceremony except that of mutual consent and in which the husband gained no legal authority over his wife. Classical law usually concerned itself only with free marriage and with *usus* which were the surviving forms. *Confarreatio* and *coemptio* were defined by the jurists of the Empire but were spoken of as generally obsolete.

Now marriage by *coemptio*, *per aes et libram*, employed a ceremony that survived for a long time in making sales. In formal sales the buyer, in the presence of five witnesses, the seller and the holder of the balances (libripens), touched the scales with a penny or a piece of copper which represented the purchase price and uttered a formula to the effect that he claimed as his the property in question for which he paid this price. When such a ceremony was employed in marriage, even though the formula was different, lawyers like Gaius naturally drew the conclusion that the rite was a survival of a bride-sale, and, inevitably, modern jurists who work with the materials of comparative institutions have readily acquiesced: Here, they say, in Roman practice is a survival from pre-Roman times when wives were purchased.

This conclusion seems to me far from convincing. In the first place, in early Latin the word *emo* meant to *take* or *accept*, not *buy*, just as our word *purchase* in Old English meant *seek* or *pursue*, not *buy*. There is as yet no proof that the word *coemptio* had anything to do with a purchase in early times. Secondly there is no other trace of bride-purchase in Roman records, whereas the word *dos* (dowry), which is very old, as the form shows, implies a practice quite the opposite of buying the bride. Again, the act of *coemptio* is sometimes

attributed to the woman as well as to the man and sometimes to both the man and the woman, in both of which cases the phrase agrees with the early meaning of the word. Furthermore, the formula, according to Gaius, was not the same as in the case of purchase. Finally, if the ceremony had ever implied a contract of sale, we should expect the maiden's father to receive the piece of bronze from the groom or from his father. There is no indication, however, that such was the case. Boethius, citing Ulpian as his source, says: Coemptio vero certis sollemnitatibus peragebatur, et sese in coemendo invicem interrogabant, vir ita; an sibi mulier materfamilias esse vellet. Illa respondebat velle. Item mulier interrogabat an vir sibi paterfamilias esse vellet. Ille respondebat velle. (In using the ceremony of *coemptio*, the man asked the woman whether she wished to become a *mater familias*. She answered "yes," and in turn she asked him whether he wished to become a *pater familias* to which he answered "yes.") These mutual questionings seem difficult to reconcile with the idea of bride-purchase. In other words we, like the classical jurists, have been misled by later usage of the word *emo* and have supported our misuse by anthropological theory.

Of course, the presence of the scales or balances and five witnesses lend some plausibility to the theory that this was in origin a sale, even though the sale's-formula was not used and apparently no coin passed. However, I take it that in the early days, before writing was known, some symbolic act that could be witnessed and remembered was desirable in making contracts. The symbol of the balances was, of course, frequently used in making purchases and might well have spread from that act to all other contracts that had to be witnessed. It came also to be used in such contracts as *nexum* and in the adoption of children and in the making of wills, though in these cases it did not imply purchase. Gaius, for instance, says that in ancient times when a man was sick and expected death he would call the person he desired to be his heir, give him his instructions and his property, and the latter, in the presence of five witnesses

would verbally accept the willed property, strike the scales, and hand the piece of bronze to the testator. He adds that this had become by his day the usual formality in making wills. Now it seems to me difficult to hold that the heir actually bought the property with the penny, for which the dying testator had no use, or that the penny represented an actual payment to be made later. This act probably never represented a sale; it was a convenient formality for the five witnesses to behold and could be readily remembered. The shaking of hands before witnesses would have served the same purpose, had it become the custom. The symbolism was clearly borrowed from one kind of contract to be used in another because before the day of writing a common formality that could be seen and recalled was needed.

I would also suggest, though with some diffidence, that when this ceremony was used in adoption—as when Augustus metamorphosed his two grandsons into sons—we may be wrong in assuming that he "bought" them from his son-in-law, Agrippa. The *mancipatio* in this case may have meant only a transfer of *manus* and the ceremony may simply have been a visible symbol of the transfer used because it could easily be remembered by the witnesses. In other words, when *coemptio* is employed in adoption, it need not be a *survival* of a rite that once meant purchase in that very transaction; it may represent merely the diffusion of a useful rite originally used in an act of purchase. Of course, the use of the words *emancipatio* and *coemptio* with reference to children led later lawyers to suppose that children were property like slaves, but it is not certain that this supposition did not rest upon ideas that arose after the words had passed through several semantic changes and no longer possessed their original meanings.

Karlowa, imbued with the idea that the coemptive formula used in marriage must have implied purchase, has invented a convenient phrase and persuaded many writers to insert it into the formula. He proposed to emend the words cited by Gellius (IV, 3) as follows: *Te ego ex jure Quiritium in manu*

mancipioque meo esse aio tuque mihi coempta esto. However, this proposed emendation on the analogy of the sale-formula is wholly inconsonant with the mutual questions and answers cited by Ulpian, with the statement of Gaius that the formula and purport of coemptive marriage differed from that of a sale, with the fact that Cicero (ejus mulieris quae coemptionem fecerit) and Gaius (I, 114, potest autem coemptionem facere mulier, etc.) speak at times of the woman as the one who *facit coemptionem*, and with the fact that Servius speaks of coemptio as a mutual transfer. If we forget anthropological comparisons with savage customs of bride-purchase for a while, and conceive of the symbolic rite of coemptive marriage as simply borrowed from the formalities used in sales, testaments, adoption, nexum, and the like, that is, as a convenient method of employing witnesses to observe the striking of the contract, we shall have no difficulty with our sources. There is no mention of a piece of bronze, probably because none passed; the act of *coemptio* is spoken of at times as being *mutual*, or when convenient, of the *mulier* or of the *vir* as performing a *coemptio*, because in this contract the give and take was equal: it was not onesided. At times, since in this old form of marriage the bride passed from the manus of the father to the husband, the word *mancipio* is used, but not in the sense of a sale, as Gaius is careful to say, but only in the literal sense of manus-acceptance. And, finally, the formula used was different from that of a contract of sale, because it had nothing whatsoever to do with sales. Only the paraphernalia were similar, and those only because they were so much in use in other contracts made before the day of writing that they were generally accessible.

These few examples will perhaps suffice to indicate the source of my belief that if the students of law will read the newer economic and social histories of Rome based upon the results of recent excavations, they will find reason to give a more patient consideration to various neglected fragments of the Twelve Tables and will arrive at a more reasonable interpretation of the contractual ceremonies described in

them. A new school of jurists has recently emerged which is advocating the study of sociology and economics in order to release the interpretation of present-day law from outworn tradition. It is also necessary, it seems to me, that students of ancient law should make a careful investigation of the social and economic conditions out of which the law grew before they undertake to explain the meaning and intention of the phrases that they attempt to interpret.

UNTANGLING ONE OF NATURE'S PUZZLES

By B. SMITH HOPKINS

(Broadcasted from Chicago by the Columbia Broadcasting System to the hall of the Society)

MUCH of the chemist's work is concerned with the problem of the preparation of materials of known purity. Most of our naturally occurring raw materials are more or less complicated mixtures of substances whose properties are similar. If from these mixtures pure chemical materials are required, the work of purification frequently becomes the chief task in meeting the demands of modern civilization. For example common salt, one of our most extensively used chemical materials is found widely distributed in the earth's crust both in the form of brine and as crystalline rock salt. It is, however, rarely found free from admixture with other materials such as rock, soil and especially other soluble compounds whose influence upon the salt is marked. Some of these compounds like the bromides give the salt a bitter taste, while others like sodium sulfate absorb moisture from the atmosphere and make the salt sticky or cause it to form a hard cake. For most purposes the removal of such impurities from the salt is demanded for very obvious reasons. Our modern standards are becoming more and more insistent that salt must be "all salt," and as a consequence the manufacturing chemist must continually improve his methods of purification. The rock and soil are easily removed by taking advantage of the fact that salt dissolves in water, consequently the simple process of filtering removes these impurities almost completely. Such treatment, however, will have very little influence upon the task of removing the soluble compounds. In the purification of salt two general methods are available: (1) addition to the brine of some chemical substance which will change the soluble impurity to an insoluble form, thus permitting its removal by filtration; and (2) the evaporation of the brine under carefully

determined conditions such that crystals of pure salt may be obtained while the undesirable impurities are separated. It is obvious that both of these steps must be taken with care, if they are to be successful. No material can be added to the brine to remove an impurity if the added substance will itself be objectionable. Moreover the amount of the compound added in this way must be definitely determined in order that an excess may be avoided. In the purification by crystallization it must be remembered that under some circumstances the impurities will crystallize before or along with the salt. Consequently the conditions must be controlled in such a way that the salt crystals may be separated as completely as possible from those substances whose removal is desired. One crystallization will never completely separate two soluble compounds, because the crystals which form first will always be wet with the solution of the other compound. Thorough washing of the crystals will aid materially in meeting this difficulty, and several repetitions of the crystallization process are effective in bringing about a reasonably complete separation. It is very obvious however that the preparation of chemically pure sodium chloride from such complicated mixtures of salts as are found in sea water is a task which presents difficulties of considerable proportions.

This simple illustration will serve to indicate the difficulties encountered in the chemist's perennial task of preparing a chemically pure substance from a naturally occurring raw material. It is true that nature in rare cases supplies us with material in a high state of purity. A clear water-white diamond represents one of the purest forms of carbon which is known and crystals of clear quartz are pure silica but these are exceptional forms of matter. Their adaptation to commercial uses requires such processes as cutting, polishing and grinding with which the chemist is not concerned, since they are purely mechanical. At the opposite extreme we find many useful substances required in a high state of purity which are won from natural sources of a most unpromising character. During 1929 the world produced over two million tons of

copper most of which had a purity of at least 99.9 per cent. Some of this useful metal was obtained from ore which contained as little as 1 per cent of copper. The world's production of gold in 1929 exceeded 400 million dollars in value, and part of this was obtained from ore which yielded as little as 1/20 oz. per ton of ore. It is also claimed that radium has been successfully won from ore so poor that one ounce of radium would require the concentration of all this precious metal that was contained in 10,000 tons of ore.

The production of high grade copper for electric wiring from a low grade ore represents a problem in intensive purification on a large scale. The concentration of high purity gold or of radium from their ordinary sources illustrates an extreme case of purification in which the dross is truly enormous. Nature furnishes many puzzles in which we are challenged to separate a useful material from an almost overwhelming proportion of waste. Many of these problems have already been solved so successfully that the results command our respect and excite our admiration. But Mother Nature has given us many puzzles which have not yet been solved; these we are apt to regard with awe and dread, but if the object to be obtained is a desirable one they should challenge our intelligence and inspire an intensive effort for their solution.

One of the most puzzling problems in purification, certainly in the field of Inorganic Chemistry, is furnished by the group of elements commonly known as the Rare Earth Group. This group may conveniently be thought of as a collection of at least fifteen elements which through some fantasy of nature have been crowded into a space which is usually occupied by a single element. All of the known elements are arranged in an orderly fashion in the systematic tabulation known as the Periodic Table. Throughout this table each element is given a single space; in a few cases three elements occupy one space and in the single instance of the Rare Earth Group do we have more than three elements crowded together into one space. These fifteen elements of the Rare Earth Group are almost

invariably all found together in the same minerals and in addition there are two others which generally accompany them. To add to the complexity of this puzzle there are, in addition to the seventeen elements which are generally thought of as comprising the Rare Earth Group, at least a dozen closely related elements which are frequently present in rare earth ores. At least three or four of these closely related elements must always be expected in every rare earth ore, and it is not uncommon to find most of them present in a single ore.

Thus it appears that the Rare Earth Group presents to the inorganic chemist a puzzle which is complicated in the extreme by the fact that the ores in which this group occurs contain from fifteen to twenty or more elements. The untangling of so complicated a puzzle as this is in itself no simple task. But when attempts are made to solve this riddle other serious obstacles are immediately encountered. While some rare earth ores are available in large quantity at a reasonable cost, most of the minerals containing these compounds are scarce and difficult to obtain. As a consequence the initial cost of the raw material is generally high and the amount available at any price is definitely limited. When an adequate supply of ore is obtained difficulties are encountered because the minerals are refractory and yield reluctantly to chemical treatment. After the material is brought into solution and attempts are made to untangle the resulting complex mixture of salts by resolving it into a series of definite individual chemical compounds, new and perplexing obstacles are encountered. When an attempt is made to apply the methods of separation which are usually employed in the purification of a chemical mixture, extremely slow progress is made for the reason that the elements of this group are very closely related chemically. Any reagent which affects one will generally react with them all and to about the same degree. As a result special methods must be applied and the best of these are lamentably lacking in efficiency. As a consequence the most useful methods of purification which

are now known are frightfully wasteful and wholly unsatisfactory.

Thus it appears that the process of untangling this complicated puzzle which nature has given us is difficult and expensive alike from the standpoint of time, money, and patience. Some members of the Rare Earth Group have been prepared in considerable quantity and in a high state of purity; these may now be purchased in the open market in large bulk and at a reasonable cost. Most of the individual members of the group have been prepared in a satisfactory state of purity, as a result of long, painstaking and patient effort. But some separations have never been accomplished completely because several portions of the puzzle remain yet to be solved. For this reason the compounds of some of the rarer members of the group are almost or quite unattainable. With this situation in mind it is easy to understand why illinium, which appears to be the rarest member of the group if not one of the rarest elements now known, escaped detection for so many years.

The methods commonly employed in the separation of the members of this group are based upon one of two facts: (1) There is a gradual, though slight, change in the solubility of the salts of these metals; and (2) there is a gradual decrease in the chemical activity of the metals as we proceed in regular order from the first to the last member of the group. According to the first fact, if we have a saturated solution of the bromates of the whole group and evaporate a part of the solvent it is obvious that some of the salts must crystallize out. If these crystals are examined and their composition compared with that of the mother liquor it is found that the crystals are richer in those members which are more difficultly soluble, while the solution contains a larger proportion of the more readily soluble compounds. If the process of partial crystallization is repeated with both the crystals and the solution, there will be still greater separations of the more difficultly soluble salts from those which are more readily kept in solution. By repeating this process of fractional crystal-

lization many hundreds of times, the original mixture of salts may be divided into a series of fractions, each one of which differs somewhat in composition from that of its neighbors. It is obvious, however, that complete separations of chemical individuals cannot be expected by this method since the differences in solubility are gradual and small. As a result fractional crystallization alone is rarely if ever depended upon to secure a chemically pure compound. The method works best for any salt which is either the most difficultly soluble or the most readily soluble of the series. Usually methods of fractional crystallization are employed as a means of splitting a large volume of a highly complex solution into a series of fractions each of which is composed mainly of one or two individual compounds, with a small admixture of one or two of the most closely related compounds. In other words fractional crystallization is the first step in untangling the highly complex Rare Earth Group.

Methods depending on the varying degree of chemical activity are of various types. If a precipitant like ammonia is added in excess to a mixed rare earth solution all the rare earth material would be precipitated. But if a very small amount of the precipitant is added only the weaker metals would be thrown out of solution. Similarly if the mixed rare earth nitrates are heated, insoluble compounds are formed; but if the heating is carefully controlled only the weaker metals will be changed to the insoluble form. These and other similar methods are used to fractionate the relatively simple mixtures obtained by fractional crystallization. In general a carefully manipulated combination of fractional crystallization and fractional precipitation or thermal decomposition will give the most satisfactory results yet obtained in the effort to untangle this rare earth puzzle.

Many other attempts have been made to find more satisfactory methods of separating the members of this group. In general they have not been as satisfactory as those which have been in use many years. New ideas are continually being developed and new methods of attack are being used.

Progress is being made in the attempts to learn more about this intricate group. Probably the most valuable new method which has recently been used is that of electrolytic reduction for which we are indebted to Professor L. F. Yntema. By this means he has prepared highly purified europium and ytterbium, two of the most rare and most difficultly separated members of the entire group.

Why should anyone be interested in untangling so complicating a puzzle? What is to be gained from such work as this? Of what possible use is it all? Such questions as these force themselves upon us when we realize that a lifetime of hard work may be expended with apparently little actual progress. There are many reasons why the scientist persists in his efforts even in the presence of discouragement and disappointment. No one can predict in advance the value of any scientific investigation. Some of the world's greatest achievements have come from the most unexpected sources. Any effort which extends the boundaries of our intellectual horizon is worth while. To the worker in the field of the rare earths there is also the conviction that perhaps this group holds the key by which we may be able to unlock the storehouse of information of the whole periodic system. If we can learn to understand the relationship of these elements to each other and to the more important commercial metals, it may be possible to use this information for a better understanding of those elements which we now consider commercially important. It is only by patient, persistent and devoted effort of this sort that scientific progress may be made. In addition the rare earth worker is stimulated by the conviction that the rare earth elements must have been put here for some specific use. It is only our own ignorance concerning them that makes them appear useless. Perhaps some one of them may prove to be extremely useful as for example as a specific remedy for a baffling malignant disease. Surely no greater contribution to modern life could be desired than one which contributes definitely to the comfort, happiness, security and welfare of the human race.

SYMPOSIUM—THE CHANGING WORLD

Section I. Tendencies in the Natural Sciences

"ASSAULT ON ATOMS"

By ARTHUR H. COMPTON

(Read April 23, 1931)

Twenty-five hundred years ago, Thales, the first true scientist of ancient Greece, undertook to solve the problem, "Of what and how is the world made?" Almost a hundred generations have passed, and the problem is not yet solved.

Democritus and his followers thought they had found the solution. Everything is made of atoms. "According to convention there is a sweet and a bitter, a hot and a cold, and according to convention there is color. In truth there are atoms and a void." Thus in terms of motions of minute particles the ancient Atomists accounted for their world. Mountains and seas, trees and people, even life and thoughts were thus explained.

But Socrates and Plato would have none of their atoms. Did they not in Democritus' hands rob men of their personality? Atoms are thus worse than useless, for they destroy the basis of morality. Here in Athens, around the question of atoms, was staged the first great battle between science and religion. Epicurus and Lucretius took up the cudgels on behalf of the atomists, but Plato carried the day, and atoms were forgotten until the revival of scientific thought during the renaissance.

Though our present day atomic theories are based on much firmer foundations than those of Democritus, they owe their origin to his ideas, transmitted down through the centuries. A few years ago we were camped beside a mountain lake in the foothills of the Himalayas, studying cosmic rays. The warm air from the plains of India was carried up over a

range of mountains, and came down again into the beautiful Vale of Kashmir. Clouds were continually forming as the air, cooled by expansion as it came up the mountain side, became supersaturated with moisture. But after passing the peak of the range, the air was warmed by compression as it sank to lower levels, and the clouds evaporated into thin air.

It was while watching such clouds in his native hills of Scotland, that C. T. R. Wilson conceived his beautiful laboratory experiments on clouds. Of course he couldn't bring the mountains into his laboratory but he could expand his moist air in a cylinder with a piston at one end. He made his cylinder of glass, in order to see what was going on. I have one patterned after his design, here in my hand. Here are the glass top and sides, with the whole vessel partially filled with inky water. There is a lamp here beside the glass cylinder so we can see better what is going on. I can compress the air in the glass chamber by squeezing the bulb. We let the air remain under this pressure for a moment, until it becomes saturated with moisture, and then allow it to expand. As it expands the air cools, and a cloud forms in the chamber just as it did on the mountain top.

Did it ever occur to you that when a cloud forms each little drop of moisture in the cloud must condense on something? Usually it condenses on a speck of dust floating in the air, and after a rain storm these dust particles are carried to the ground and the air is beautifully clear. But when the dust has been removed, what can the drops condense on? There are always in the air some broken bits of atoms and molecules, which we call *ions*. These ions are produced by rays from radioactive substances in the ground, and other sources. So Mr. Wilson tried the experiment of placing a speck of radium in his expansion chamber, to see what kind of clouds would be formed. Let's see what happens when we repeat his experiment. Those of you who are near enough will see the little white lines radiating out from the tip of the glass rod which carries the radium. These little white lines are tiny clouds of water drops, condensed on the ions

left along the paths of particles shot out by the radium. It is clear that particles of some kind are coming from the radium. What are they?

For those of you who are too far away to see these little clouds, let me show a series of photographs of what is happening in this chamber. First you see a picture taken from above (Fig. 1). Here are the glass walls of the chamber, and here is the rod on which the speck of radium is placed. These more

FIG. 1.—Tracks of alpha particles (helium atoms) made visible by condensing clouds along their paths. (Wilson.)

or less diffuse lines are the clouds of water drops that mark the paths of the particles ejected from the radium.

What are these particles? Let us call them *alpha* particles, in order not to imply anything about what they are, and look into their properties. Fig. 2 shows a sharper

FIG. 2.—Helium atoms ejected from radium.

photograph, each line a thin straight cloud, marking the path of an alpha particle. Rutherford (just recently made Lord Rutherford in recognition of his work with atoms) caught a large number of these particles, to find out what they were when there are enough of them to handle. Niton is a radioactive gas, a hundred thousand times as active as radium. He compressed some of this gas into a fine glass tube with walls so thin that the alpha particles would pass right through. After a few days he noticed gas collecting in the space surrounding this tube, and this gas he forced into a fine tube above. On passing an electric discharge through the tube and looking through a spectroscope at the light emitted, he saw the brilliant spectrum characteristic of the gas helium.

Many of you know the romance of helium. Observed many years ago by Lockyer in the spectrum of the sun, it remained unknown on the earth for a generation until Rayleigh and Ramsay, making a precise measurement of the density of the nitrogen in the air, found it different from the nitrogen prepared in the laboratory. Search for the cause of the discrepancy revealed a whole series of new gases—argon with which our incandescent lamps are filled, neon with which we advertise our wares in blazing red, helium with which we now fill our dirigibles, and two others, krypton and xenon which are now of great value in certain laboratory experiments. Thus was helium found, and here we see it being formed—the birth of helium atoms. For these alpha particles are none other than atoms of helium gas.

We can count these atoms one by one as they come from a preparation of radium. It might be done using an expansion chamber of this type, and counting the tracks as they appear. A better method is to allow the atoms to enter an electrical counting chamber. Each particle then can make its record on a moving film, as we see in Fig. 3. Every little peak here marks the birth of a helium atom from its parent radium.

Imagine that we have thus counted all the atoms of helium that come through the walls of Rutherford's glass tube, and make the gas that he observed in his spectroscope. How

many atoms would we have? In a little glass bulb the size of a large pea, filled with helium at atmospheric pressure, the number of atoms is about 1 with nineteen ciphers after it.

FIG. 3.—Counting atoms. Each peak marks the entrance of one helium atom into the counting chamber. (Geiger and Rutherford.)

Perhaps that doesn't mean much to you. Let me put it this way. Two thousand years ago Julius Caesar gave a dying gasp, "Et tu Brute?" In the intervening millenniums the molecules of air that he breathed out with that cry have been blown around the world in ocean storms, washed with rains, warmed by the sunshine, and dispersed to the ends of the earth. Of course only a very small fraction of these molecules are now in this room; but at your next breath each of you will probably inhale half a dozen or so of the molecules of Caesar's last breath.

Molecules and atoms are very tiny things; but there are so many of them that they make up the world in which we live.

The Parts of the Atom

The story is told of Lord Kelvin, a famous Scotch physicist of the last century, that after he had given a lecture on atoms and molecules, one of his students came to him with the question, "Professor, what is your idea of the structure of the atom." "What," said Kelvin, "The *structure* of the atom? Why, don't you know, the very word 'atom' means the thing that can't be cut. How then can it have a structure?"

"That," remarked the facetious young man, "shows the disadvantage of knowing Greek."

Does the atom have parts?

The Electron

Do you see the faint little trail at the bottom of Fig. 4? It appears to be due to something much smaller than the

Fig. 4.—Trails of alpha and beta particles.

particle which made the broad bright trail above it. If we called the one an alpha particle, let us call the other a beta particle, and try to find out what kind of thing it is.

Fig. 5 shows a large number of these beta particles, that have been knocked out of air molecules by the action of

Fig. 5.—Beta particles, ejected from air by X-rays.

X-rays. You can see where the X-rays passed through the middle of the chamber. Now every substance has its own peculiar kind of atoms. Iron atoms differ from oxygen atoms,

and these from atoms of carbon and so on. But these beta particles are all alike, as far as we can tell, and they can be knocked out of anything. Had we put into the chamber fried eggs or a platinum wrist watch, the same kind of beta particles would have been observed. Thus beta particles are things which go to make up all kinds of matter. They are more fundamental even than atoms.

But what are these beta particles? In the first place they carry an electric charge. Notice in Fig. 6 how their trails are curled up if a magnet is held near the expansion chamber.

FIG. 6.—Beta particles (electrons) curved by magnetic field. (Skobeltzyn.)

This is because the moving electric charge acts like a wire carrying an electric current, and the particles form the armatures of tiny electric motors.

Professor Millikan, a member of our society, spent years at the University of Chicago in measuring the charge carried by one of these little particles. He built himself an electroscope in which a tiny drop of oil took the place of the usual gold leaf, and he would catch these beta particles on his oil drop. Every particle carried the same charge, he found. It was also the same charge that a hydrogen ion carries when water is dissociated into oxygen and hydrogen by the passage of an electric current.

Because it carries this unit of electric charge, which seems

to be an indivisible unit, these beta particles were called *electrons*, and by that name they have become familiar.

These electrons have been weighed, too, and their weight is found to be very small indeed. The atom of hydrogen is the smallest atom we know, and as we have seen, it is a very tiny thing. But an electron weighs only 1/1845 as much as does a hydrogen atom. Thus we were correct in guessing that the beta particle which made the faint trail was much smaller than the alpha particle that made the broad bright streak on an earlier photograph.

The electron is indeed one of the components of which the atom is built. We can in fact count the number of electrons that each atom has. Hydrogen has 1 electron, helium 2, lithium 3, and so on. Oxygen has 8 electrons in each atom, iron 26, and uranium, the heaviest atom of all, has 92 electrons.

The Nucleus and the Proton

But this is only a part of the story. The electrons are all particles of *negative* electricity. The atom itself is electrically neutral, and must therefore have in it some positive electricity to neutralize the negative electrons. If time were available, I should describe for you the beautiful experiments carried out by Rutherford and Aston in Cambridge, Dempster at the University of Chicago, and others, which have shown that this positive electricity is concentrated in a very small nucleus, which though much smaller in size than the atom has yet nearly all the atom's weight.

The careful experiments of Dempster and Aston have shown that the weights of the nuclei of the various atoms such as oxygen, nitrogen, sodium and the rest, are whole multiples of a unit which is nearly equal to the weight of the hydrogen nucleus. This suggested that the various atomic nuclei are built up of hydrogen nuclei. This idea was supported by the fact that the electric charge carried by the various atomic nuclei is always an integral multiple of the charge carried by hydrogen nucleus.

Many attempts have been made to make one element out

of another. This is in fact the old problem of alchemy, to make gold out of lead. The first success was got by Rutherford. He didn't get gold out of lead; but he did get hydrogen out of nitrogen and out of aluminum and other elements.

The experiment can best be shown using again our cloud expansion apparatus, as has been done for example by our fellow member, Professor Harkins. Fig. 7 shows a group

FIG. 7.—Alpha particle knocking hydrogen nucleus out of nitrogen atom (Blackett.)

of alpha particles shooting through nitrogen gas. Most of them go straight to the end of the path, and this is remarkable, for each alpha passes right through tens of thousands of nitrogen atoms before its flight is stopped. But here we see a really surprising occurrence. The alpha particle dives into a nitrogen atom, and out of it emerges a smaller particle, which goes out leaving a thin straight trail. The nitrogen nucleus with the alpha particle now attached moves heavily along in a different direction. The alpha particle has served as a hammer to knock a hydrogen nucleus out of a nitrogen atom.

Similar experiments have been done with many other elements, and most of the lighter ones have thus been disintegrated, expelling always a hydrogen nucleus. Thus we may take this nucleus, like the electron, as a component of which the various atoms are built. We give to the hydrogen nucleus now the name of *proton*, *i.e.* the original or fundamental thing. Out of protons and electrons we believe all the ninety-two different kinds of atoms are built.

How the Atom is Built

Old Ptolemy, the ancient Greek astronomer, knew that there was a sun and a moon, the earth and the planets; but he didn't know what the solar system is. When Copernicus and Galileo showed however that there is a sun, around which revolve planets in definite orbits, then men felt that they had become acquainted with their world. So, though we have found the parts of which the atom is made, we really don't know the atom until we know how these parts are put together.

Perhaps the best way to find out how something is made is to look at it. If it is something like a watch, which we can hold in our hands, this is comparatively easy. If it is the cell structure of a muscle that we wish to examine, we put it under a microscope. But some things are too small to see even in a microscope. By using ultraviolet light of wave-length shorter than ordinary light, we can photograph such things as typhoid bacilli with increased sharpness. But atoms are too small even for this.

Now X-rays have a wave-length only a ten thousandth that of light, and if we could use them in a microscope it should be possible for us to observe even the tiny atoms. Unfortunately we cannot make lenses that will refract X-rays, and even if we could, our eyes are not sensitive to X-rays. So it would seem that we shall never be able to see an atom directly.

It is nevertheless possible for us in the laboratory to get by more round about methods precisely the same information about an atom that we should if we could look at it with an X-ray microscope. I have spent a large part of the last sixteen years trying to find what the atom looks like, and it has become something of a game with me.

Last summer while spending a brief vacation in northern Michigan, I noticed a fuzzy ring, not very large, around the moon. Half an hour later the ring was perceptibly smaller, and within an hour we had to come in out of the rain.

This ring was due to the diffraction of the moonlight by

tiny water droplets that were beginning to form a cloud. The size of the ring depends upon the size of the water drops—if the drops are small, the ring is big, and *vice versa*. So when the ring grew smaller it meant that the drops were growing larger. Soon they would fall as rain.

Our method of studying atoms is very similar to this method of finding out the size of the droplets in a cloud. Instead of the moon we use an X-ray tube, and in place of the cloud of water droplets we use the atoms in air or helium. For the wave-length of the X-rays bears about the same ratio to the size of a helium atom that a light wave bears to a droplet of water in a fog. The helium atoms spread the X-rays out into a halo. This halo, now of X-rays scattered by the helium atoms, corresponds precisely to the ring around the moon diffracted by the cloud droplets. Likewise here, from the diameter of this halo, we can estimate the size of the helium atom. We can also tell pretty much what it looks like, just as if the atom were under the microscope.

Fig. 8 shows how the helium atom would look if we were to see it with an X-ray microscope. The picture

FIG. 8.—"Appearance" of a helium atom, as found by X-rays. (Langer.)

is drawn carefully from the data we have got from the diffraction haloes. Of course it is highly magnified, about a thousand million times. Such a magnification would make a pea appear as big as the earth.

In the middle of this fuzzy ball somewhere is the nucleus of the helium atom, which has in it the protons. This fuzzy atmosphere is due to the electrons. We noted above that the helium atom has only two electrons in it. You may wonder how with only two electrons the atom can seem so diffuse. Did you ever see the boys on the Fourth of July waving the sparklers to make circles or figures eight? Of course the sparklers weren't in the form of circles; they appeared that way because they moved so fast. So here, the electrons give this continuous, diffuse appearance to the atom because they are now here and now there, and we have caught a "time-exposure" of their average positions. This is of course what we would see if we could look at the atom.

There have been fifty-seven varieties of atomic theories proposed. Lord Kelvin thought the atom was something like a smoke ring; J. J. Thomson said it was a sphere of jelly. Rutherford called it a miniature solar system, while Bohr and Sommerfeld calculated precisely the orbits of the planetary electrons revolving about the central nucleus. Lewis and Langmuir objected, and said the atom is a cube. "Not so, it's a tetrahedron," claimed Lande. "Quite a mistake; it's a diffuse atmosphere of electricity around a central core," says Schrödinger. "Only it isn't diffuse electricity," complains Heisenberg, "It's electrons moving now here, now there, which make up this atmosphere."

Each of these theories has found support in that it has explained certain physical or chemical or spectroscopic properties of atoms. For the most part, each theory has been better than the one before, because it has explained the things which the earlier one described and some new thing as well. It may seem over optimistic to suppose that there is anything final about the most recent theory. Yet the fact remains that there is one and only one such picture, namely that of Heisenberg, that describes what we find when with our "X-ray eyes" we look into the atom.

Does this mean that the problem of the structure of the atom is solved? Not yet! We feel that we know in general

outline what this electron atmosphere of the atom is like; but there's the nucleus of the atom. What is it like?

"What's the idea of bringing that up?" you ask me. "Surely that little nucleus isn't big enough to amount to anything!"

It is the nucleus of the radium atom from which the alpha particles came. Did it occur to you that those alpha particles carry a tremendous amount of energy? It is about a million times as much as is released when a molecule of T N T explodes. It is only because they are liberated one at a time that the alpha particles make so little impression.

Did you ever pause to wonder where all the energy of the sun comes from which it is pouring out as heat? If it were made of pure coal burning in oxygen, the sun could shine with its present brilliance for only a few thousand years, less than the era of history, before it would be reduced to a cinder. Even if it were composed of uranium or radium, and got its heat from their disintegration, it would last only for a few billion years, which is about the age of our own earth; yet our geological records indicate no change in the sun's brightness over this vast period. The best astronomical evidence indicates that the sun must be at least a thousand billion years old. What is the enormous supply of energy which has kept it hot for so long a time? Professor McMillan has pointed out that apparently the only way to explain the sun's long life is to suppose that the sun is consuming itself. If under the extreme pressure and temperature of the sun's interior the electrons and protons in an atom should come together and neutralize each other, all of their energy would be liberated and add to the sun's heat. Such a process would release energy almost beyond belief. From five drops of water, if we could thus squeeze out all the energy, we should be able to run all the power stations in Philadelphia for twenty-four hours.

Is it possible for man to tap these great stores of energy? We do not know. We know the energy is there, and the evidence is strong that it is being liberated in the sun and

stars. But under what conditions? Perhaps we cannot realize the proper conditions here on the earth. In any case it is our job—the physicists job, that is—to find out whether this energy can be used, and if so, how.

If we are to find the conditions for the release of these vast stores of energy, we must acquaint ourselves with the atomic nucleus, for it is there that the energy lies. Studies of the band spectra of molecules have shown us something about the rotation of the nucleus. The masses of the nuclei and their electric charges have been measured by the help of magnetic spectrographs and scattered X-rays. Attempts have been made to disintegrate atomic nuclei by bombardment with high speed electrons shot by high voltages. But by far the most fruitful tool for studying the nucleus has been radioactivity.

Experiments with scattered alpha rays have shown the minute size and relatively large mass of the nucleus. They have enabled us to measure its charge, and even to estimate the field of electric force in its neighborhood. Further information on the latter point is given by the speed with which the alpha particles are ejected from the radioactive nucleus. Combining the evidence from these alpha ray experiments, it becomes evident that surrounding the nucleus there is a "potential wall," which prevents alpha particles that are outside from entering the nucleus and those on the inside from escaping. We are thus afforded a basis for developing a quantum theory of radioactive disintegration according to which the probability of an alpha particle jumping this wall is greater if it has large energy, and a qualitative explanation of one of the fundamental laws of radioactivity is obtained. Studies of the sharpness of gamma ray lines suggest a nucleus in which planetary alpha particles correspond to the electrons of the outer atom; though how these particles are held together remains unknown. Similarly the condition of the electrons in the nucleus remains unsolved. There is no gamma radiation that can be traced to these electrons, and when they appear as beta particles their energies are dis-

tributed over broad bands. Though much new light is shed by these studies in radioactivity, the nucleus of the atom, with its hoard of energy, thus continues to present us with a fascinating mystery.

Thus our assault on atoms has broken down the outer fortifications. We feel that we know the fundamental rules according to which the outer part of the atom is built. The appearance and properties of the electron atmosphere are rather familiar. Yet that inner citadel, the atomic nucleus, remains unconquered, and we have reason to believe that within this citadel is secreted a great treasure. Its capture may form the main objective of the physicists' next great drive.

HOPES IN THE BIOLOGICAL SCIENCES

By WILLIAM MORTON WHEELER

(Read April 23, 1931)

My topic, as worded, appears to be somewhat ambiguous. It would seem to refer either to the hopes of the general public concerning the future advantages to be derived from biological research, or to the hopes entertained by the biologists themselves in the outcome of their labors. Since I am by no means certain that the public has any definitely formulable hopes of biology, except, perhaps, those relating to relief from certain terrible diseases, I will, with your kind permission, consider only what I conceive to be the main hopes of the investigators of living organisms. Perhaps these hopes, if realizable, might be regarded as generally satisfactory. Before proceeding, however, I should like to give the subject a more definite setting.

St. Augustine informs us that when the actor who impersonated Chremes in Terence's adaptation of Menander's comedy, the "Self-tormentor," uttered the line *homo sum, et humani nil a me alienum puto*—I am a man and I deem nothing that is human foreign to me—the Roman theatre resounded with applause. Some scholars imply that it was only the actor's consummate art that elicited this response, because the sentiment must have been trite even in 163 B.C. Indeed, the line may have occurred in the lost original play written by Menander in his youth, about 324 B.C., and the Stoic doctrine of the universal society of mankind had long been familiar. Perhaps, however, Terence's verse may have had a certain freshness, which we who live in a much more highly integrated society are unable to feel. At the present time, at any rate, we are all so interdependent that, as George Boas says, "we want to live the lives of others; we want others to live our lives. . . . The newspaper interview, pub-

licity, the radio, the social worker, the autobiographical novel, organized play, *tours en masse*, "just one big family," we live the life of one of those marine animals whose nervous system is a nervenet; when one bit of it is stimulated, the whole shudders in sympathy."

The depth and universality of this feeling that nothing human is foreign to us is shown most impressively in those vast accumulations of knowledge and unverified inference which constitute what we call the humanities and the Germans call the 'Geisteswissenschaften,' the sciences and pseudosciences of theology, metaphysics, epistemology, ethics, esthetics, psychology, history, sociology, economics, law, politics, linguistics and education. All of these have been ardently cultivated in the past by men who tacitly assumed that a complete knowledge of man could be secured by studying him as a unique and isolated species. Their interests were therefore exclusively or at any rate very largely *anthropocentric;* their motto that of Terence, the Protagorean "man is the measure of all things," or Pope's aphorism, "the proper study of mankind is man." To the biologist of today this interest seems to be too narrow, for the reason that our knowledge of any organism must be distorted and inadequate unless its genetic and environmental relationships are given due consideration, and this obviously includes a scientific, nonvaluative, comparative study of existing man as only one of a vast series of extinct and contemporaneous organisms. Biologists are therefore *biocentric* and would be inclined to expand Terence's verse to read: "I am a living organism and I deem nothing that is living foreign to me." The Hindoos many centuries ago adopted this attitude from belief in metempsychosis; biologists have adopted it only within the past seventy years from a conviction of the truth of organic evolution. That it is not confined to biologists is shown by the increasing interest of all civilized nations in the conservation of wildlife, in the active developments of horticulture, of zoological and botanical gardens, museums, etc.

But, after all, biology deals only with a small fragment of

reality—with that thin and discontinuous film of living matter which grows and proliferates on the land surfaces and in the waters of our rather diminutive planet. Poets and pantheistic philosophers, no less than geologists, chemists, physicists and astronomers have an even more expansive, *cosmocentric* interest, and their version of Terence's line would, perhaps, read: "I am a space-time event and I deem nothing that is a space-time event foreign to me." Remote and tenuous as such an interest may seem, and without going as far as Groddeck when he says that "if one wished to utter one unquestionable truth about humanity, one would need to know the whole cosmos," we gladly admit that we still have much to learn about the biochemistry, biophysics and the cosmic significance and destiny of man.

Of course, we can not regard the three interests, which I have mentioned, as mutually exclusive. They obviously represent only so many natural expansions of our abiding interest in ourselves, an interest which we hope will yield a deeper, more satisfactory and more useful knowledge of mankind in general. The biocentric certainly owe much to the older anthropocentric sciences, which have provisionally, and often erroneously, formulated essentially biological problems like those of the relations of form and function, body and mind. Certain statistical methods of handling data, and conceptions like the survival of the fittest, the struggle for existence, organic differentiation as the result of division of labor, are known to have crept into the biological from the social sciences. On the other hand, we see peculiarly biological formulations, like that of the 'organism,' invading the theories of modern physicists, chemists and astronomers.

Now what are the hopes of the biologist? They are, no doubt, many and diverse and in part vague and inarticulate, but there seem to be two of which he is clearly conscious. He hopes, first, to obtain from investigation accurate, verifiable data that can be utilized in establishing a more adequate knowledge of the essential peculiarities of organisms, and second, he hopes to see this knowledge more extensively

utilized in human thinking and behavior. He is encouraged in these hopes by his knowledge of the history of research. In the biological sciences, at any rate, this history is so recent that many of my audience have witnessed not only the development of whole sciences from casual or apparently insignificant bits of investigation, but also the profound and beneficent effects of this knowledge on the anthropocentric sciences and human behavior. In fact, the history of the dozen or more biological sciences, like that of the other natural sciences, abounds in such instances, and even the humblest investigator hopes to initiate or to aid in initiating similar developments though he is, as a rule, quite unaware of their future possibilities. Who could have foreseen, for example, that the doctrine of organic development and evolution, that key-idea of our modern civilization, as Overstreet calls it, would arise from Lamarck's classificatory studies of animals and plants and Darwin's and Wallace's casual observations on the distribution of certain groups of tropical animals? Or who, in 1839, could have foretold that Schwann's rudimentary studies of plant-cells would culminate in our modern cytology, histology, embryology and pathology? The whole science of genetics has arisen unpredictably since the beginning of the century from two sets of observations—those of Mendel, in 1865, on the hybridization of peas—overlooked for more than 30 years, and those of my old teacher, Edouard Van Beneden, in 1883, on the chromosomal equivalence of the male and female pronuclei in the egg of the roundworm of the horse. Bacteriology and immunology can be traced back through the splendid achievements of Pasteur, Koch, Jenner and others to the almost playful observations of Leeuwenhoek in 1682 on some microbes scraped from his own teeth. Even as late as 1890, when Theobald Smith discovered the cause of Texas cattle-fever, could anyone have predicted our present knowledge of insects as carriers of pathogenic organisms, the successful completion of the Panama Canal and the sanitation of great tropical regions? Recent investigators of animal behavior like Sherrington,

Pavlov, Thorndike, Yerkes, Watson and Koehler were probably aware of the general significance of their experiments, because they were performed on higher mammals and human infants, but A. P. de Candolle, when he studied the heliotropism of plants in 1835, did not foresee the generalizations of Verworn, Loeb, Jennings and Parker, nor were such early naturalists as Réaumur, Bonnet, François and Pierre Huber aware that their careful studies of the instinctive behavior of insects would have such a direct bearing on our understanding of the human drives and appetites, as appears from the recent work of Holt, Legewie and others. Another instance, though of purely biological interest, is the development of our modern procedure in the classification of organic forms. In the more thoroughly studied groups of animals, the mammals, birds, mollusks and insects, the species of the early classifiers are now resolved into form-cycles ("Formenkreise"), or complexes of more or less variable geographical races, or subspecies. Strangely enough, the initiator of this procedure, which goes some distance towards reconciling taxonomy and genetics, was the philosopher Emmanuel Kant who, in 1775, first introduced it into his account of the distribution of human races. It was independently advanced six years later by Esper in his studies on butterflies. After being overlooked for nearly a century it was resuscitated by American and German systematists and is now yielding data for a really scientific study of organic evolution and geographical distribution.

Now since the anthropocentric actually merge into the biological sciences through the group of anthropological sciences, and since, moreover, all three groups have a common subject matter so far as man is concerned, it is not surprising to find that any important biological theory rarely remains confined to the field in which it originated but promptly invades the anthropocentric group. There it begins to act like a ferment, or catalyst on all our wishful thinking and time-honored, unverified assumptions and dogmas. Even if it be true that the universal unrest of our civilization is in

great part due to economic conditions and the marvellous developments of technology, we must also admit that the constant increase in our biological knowledge is a very important contributory factor. Not one of the anthropocentric sciences has escaped this influence. In consequence, some of them are adopting a frankly biological orientation, others show their uneasiness in their complaints that the biologists are endeavoring to pasteurize not only the milk of humanistic knowledge, but even the milk of human kindness, and yet others are resisting the biological ferments with all the devices of a host-organism invaded by parasites. In the last group is theology, which is rapidly succumbing to the inroads, not only of the biological and physical sciences, but even of some of its former anthropocentric servant-sciences, history, anthropology and psychology. Its status in the curricula of our higher secular institutions of learning is already that of a vestigial organ, and prophets venture to predict that before the end of the century it will have no more cultural value than astrology. Metaphysics is faring no better, and is being exposed by the philosophers with slight regret, like a non-viable Spartan infant, while they busy themselves with the theory of knowledge and the religious, social and educational problems that have become acute, as the result of psychological and physiological research. The old formal logic has reached its dotage, and is yielding its place to a new non-Aristotelian, biophysical logic. Ethics is torn between the conservative old moralists, who hold fast to their supernatural sanctions and injunctions and a radical, youthful faction insisting that moral codes shall be based on life and not life on moral codes. The social sciences, like insect larvæ, are in a stage of ecdysis, struggling to rid themselves of their valuative epidermis and to emerge as genuine biosocial sciences. Law and political science, recently estimated to be more than 2700 years behind applied natural science, unfortunately still suffer from some deep-seated disorder of the ecdysial glands, so that there seems to be no immediate prospect of a successful moult. Our traditional, academic

psychology, though at present in a high fever, is undergoing a copious blood-transfusion from physiology, behaviorism and psychoanalysis.

Did time permit, this biologizing of the anthropocentric sciences might be shown to extend also to literature and the fine arts. That it will continue is one of the fervent hopes of biologists. There is, however, a more indirect and subtle influence on humanistic thinking, through the effects on our behavior of the *applied* biological sciences, which range all the way from forestry and agronomy to eugenics. Those exerting the most powerful and salutary influence at the present time are medicine, hygiene, epidemiology and psychiatry. It seems not to have been generally noticed that all the applied biological sciences are really so many departments of biological engineering and that they are essentially only applications of ecology, since they involve scientific regulation of man's relations to his biotic, social and cosmic environments. But ecology is, in turn, rooted in physiology, neurology and behaviorism and has therefore developed a conception of man quite unlike the encomiastic, valuative conceptions of the anthropocentric sciences. However humiliating it may be, the biologist insists that we are fundamentally not so many indivisible, immaterial, immortal souls bombinating in the cosmos, but so many very unstable lumps of juicy colloids, largely in the form of wonderful sensory, nervous, muscular and glandular tissues, with elaborate alimentary and circulatory systems to nourish them and mineralized skeletons to hang them on and make it possible for them to act on the external world. These and some additional materials, including several meals a day, are essential to the production and operation even of a philosopher, and, as Hogben says, "when the philosopher has finished all he has to say about Nature and Life, it is the biologist who is called in by his relations to certify that he is legally dead."

Furthermore, no matter how flattering and mysterious what transpires between our ears may seem to us, our overt

behavior, apart from its greater complexity, is in last analysis like that of all the other higher animals. It always consists of neuromuscular or neuroglandular responses to internal or external stimuli and of ceaseless efforts of the organism to adapt itself to its constantly changing outer world (Umwelt). That this is a basic truth physiologists, ecologists, behaviorists, psychologists and psychiatrists unanimously maintain. From this point of view, "the differences between *thinking*, *willing* and *doing* are far less significant than the identities, for all are modes of response" (Holt). Even our random movements, maladjustments, diseases and death itself are merely so many, albeit unsuccessful, reactions to stimuli.

It is not surprising, therefore, that all of us are finding it increasingly difficult to react sanely and efficiently to our extremely intricate twentieth century social and economic environments and that many of us give up the struggle and lapse either into infantile patterns of behavior or revert to those of our troglodyte ancestors. Where, indeed, with the disintegration of traditional religion and ethics, can we hope to find the means of correcting our mental, moral and physical maladjustments, except in a biologically renovated ethics and a system of education imbued with the achievements of hygiene, psychotherapy, endocrinology and genetics?

To what extent will the biologist's hopes of a permanent influence of biological knowledge on our behavior and welfare be fulfilled? He sees all departments of chemical and physical engineering receiving an ever increasing, enthusiastic welcome from the public and a more moderate appreciation of the applications of the biological sciences which are concerned with forestry, agronomy and medicine. But those which deal with eugenics, sex-hygiene and voluntary limitation of the population encounter such a resistant barrier of emotions, prejudices and ancient mores that their general acceptance will probably be long delayed. So far as these matters are concerned, therefore, the biologist will have to possess his soul in patience. He will remember the history of the doctrine of evolution which, for very similar reasons, after more than

half a century of confirmation, is still anathema to some of our institutions and the object of adverse legislation in some of our commonwealths. Indeed, the present hopes of eugenics are even less promising than were the hopes of evolution during the last decades of the nineteenth century, because evolution was mainly concerned with a reorientation of human thinking, whereas eugenics, as applied genetics, demands action. Of the eventual success of at least a part of its program, however, there would seem to be every prospect.

LENGTHENING THE SPAN OF LIFE

By LEE K. FRANKEL

(*Read April 23, 1931*)

Mr. President: This is not the first time that the question of the span of life has been presented to the American Philosophical Society. In the *Transactions* of the Society for the year 1793 a letter was published from William Barton, Esq., to the then President, David Rittenhouse, LL.D. Apparently this was the formal way of presenting communications to the Society. The letter was entitled "Observations on the Probabilities of the Duration of Human Life and the Progress of Population in the United States of America." An interesting commentary on the letter, which is dated Philadelphia, March 17, 1791, one hundred and forty years ago, and which fills sixty-one pages of the Proceedings, are the concluding lines which read, "I am, Dear Sir, with great respect, your affectionate nephew, W. Barton."

In reading this very interesting communication, it appears that the primary purpose which Mr. Barton had in view was to furnish proof that the United States was the most desirable place in which to live. In the opening paragraph the author stated that the duration of human life, the progress of the population, and the causes which accelerate that progress were unparalleled elsewhere. In proof of this he compared conditions in the United States with those of certain European countries, which showed, in his belief, the advantages on the side of the United States.

Among the causes which Mr. Barton assigned were the greater salubrity of the climate, the great fruitfulness and resources of the country, the consequent facility of acquiring the means of a comfortable subsistence, early marriages, and the virtuous and simple manners of the great body of the inhabitants. He quotes Benjamin Franklin as follows:

"With us in America, marriages are generally in the morning of life. Our children are therefore educated and settled in the world by noon. We have an afternoon and evening of cheerful leisure to ourselves. By these early marriages we are blessed with more children, and from the mode among us—founded in nature—of every mother suckling and nursing her own child, more of them are raised. Thence the swift progress of population among us—unparalleled in Europe."

Mr. Barton did not confine himself to mere statements, but attempted to prove his contention by statistical data. Attached to the communication are tables showing the probabilities of the duration of human life from birth to ninety years of age, and for divers intermediary periods in Philadelphia, as contrasted with cities in Europe. From the table for Philadelphia for the years 1782, 1788, 1789, and 1790, made up from the registers of Christ Church and St. Peters in Philadelphia, it appears that of 1000 children born, 611 were alive at age 3, whereas in London only 492 were alive, and in Vienna only 431. At age twenty, 400 were alive in Philadelphia, but only 272 in London, and 247 in Vienna. In Philadelphia 140 were alive at age fifty, while only 97 were alive in London, and 96 in Vienna. And finally at age eighty, 6 of the 1000 were still alive in Philadelphia, and only 2 in each of the other cities.

Whether the data which Mr. Barton gives are reliable and accurate it is difficult to say. Nevertheless, it is upon these statistics that Mr. Barton glows with pride. The fact that 611 children out of 1000 born were still alive at age three is a testimony which the author adduces to prove how much more carefully babies were nursed and reared in Philadelphia than in European cities, and the fact that out of 1000 individuals 6 reached age eighty, as contrasted with 2 for either London or Vienna, was adequate proof that salubrity of climate and general living conditions were favorable to a long duration of human life.

The data of Mr. Barton's are approximately those of

Philadelphia of one hundred and forty years ago. I have tried to picture to myself what Mr. Barton would say could he transport himself from the eternal shades and be present at this meeting. I can imagine his amazement at learning what has happened in what is, after all, a comparatively short period in world history. Mr. Barton might even question the accuracy of a life table of Philadelphia based on the 1920 Federal Census which shows that out of 1000 births, 890 were alive at age three, as contrasted with the 611 of his table; 836 were alive at age twenty, as contrasted with his 400; 657 were alive at age fifty, as compared with the 140 of the previous table; and instead of 6 being alive at age eighty, actually 133 were alive. The reverse of the picture is equally significant. To us it seems incredible that in those early days of the United States nearly 40 per cent of the children born in any year should have died before they were three years old, and that out of each such thousand only 6 should arrive at the venerable age of eighty.

What has brought about these marked changes? At what period in the interval since 1790 did they begin? Are they due primarily to the causes which Mr. Barton assigned—salubrity of climate and good living conditions—or have new factors arisen which have markedly influenced the death rate?

The answer is obvious. What has happened has happened largely in the last fifty years. It has been due primarily to researches in medical science, and in particular to that great outstanding discovery of the bacterial origin of certain diseases. It is no exaggeration to say that more has been accomplished in the reduction of the death rate throughout the world in the last fifty years than occurred in the previous five thousand years. In concrete terms, what has this discovery meant?

May I give a personal observation? I was a student in the laboratory of the late Dr. Henry Leffmann in the summer of 1885. There was an epidemic of typhoid fever in Plymouth, Pennsylvania, which in a population of 8,000 caused 1,104 cases and 114 deaths. Dr. Leffmann had been

called in to make the chemical analyses of water conventional at that time. The results on the chemical side were negligible. Dr. L. H. Taylor of Wilkes-Barre, during the summer of 1885, traced the epidemic to pollution of the stream feeding the Plymouth water supply. The epidemic was charged by Doctor Taylor to a case of a patient who presumably contracted the disease in Philadelphia, and who returned to Plymouth in January, 1885. That Doctor Taylor's investigation changed Doctor Leffmann's conception of typhoid fever is shown by Doctor Leffmann's own paper before the Philadelphia County Medical Society on November 24, 1885. Here, Doctor Leffmann said: "There is a large and, I think, increasing number of sanitarians and physicians who regard the disease as strictly a germ disease, that is incapable of originating except from a previous case."

Typhoid fever had been heavily and widely prevalent in Philadelphia for many years prior to the Leffmann-Taylor inquiry into the Plymouth situation. In 1865, the typhoid death rate was 1,250 per million of population; in the period 1880 to 1884 the rate averaged 682 per million. That the water supply was suspected for many years to have had an important bearing upon the prevalence of typhoid fever in Philadelphia is shown by the effort in 1872 to guard the Schuylkill River against pollution and by the prohibition of privies and wells within the city limits (1873). In 1896, the late Mr. Allen Hazen presented his plans for a filtration system and in 1902 the lower Roxborough plant and in 1904 the Belmont plant were supplying filtered water to parts of the city. Not until 1911 was the entire city supplied with filtered water. Declining typhoid fever incidence was noted in those sections of the city successively supplied with filtered water. Chlorination of Philadelphia water was introduced in 1913 and bacterial examination of milk in 1899.

What were the results of this effort to bar the typhoid bacillus which first became known to Philadelphia doctors and sanitarians in December, 1880, through the Medical and Surgical Reporter? The gradual coverage of the whole city

with clean water, and later with disinfected, clean water, was followed by a gratifying decline in typhoid fever incidence and deaths. In the period 1900 to 1904, marking the beginning of filtered water, 29,190 cases and 3,182 deaths were reported at a rate of 491 deaths per million population. Following the completion of the filtration project in 1911, the death rate from typhoid fever in the period 1912–1916 was 100 per million. In 1930, following the systematic use of filtered, chlorinated water, pasteurized milk, food inspection, and other measures, only eighteen deaths occurred, or a death-rate of 9 per million! This was not due to any greater salubrity of climate nor to the fact that my fellow townsmen were living more orderly and hygienic lives, but to the discovery of the typhoid bacillus.

I have mentioned this dramatic incident, since it tells the story of what has happened throughout the world in the last fifty years. Year by year since that time the specific bacterial and parasitic causes and origins of certain diseases have been found. Year by year since that early day medical science has plodded and striven in the laboratory, in the clinic, in the hospital, and in the field to discover the causes of disease and to find the means of their prevention. It would be idle for me to elaborate the category of accomplishments. They are known today even to the school child.

How different is the situation from that which Mr. Barton described in his very interesting document, when he stated that out of 198 deaths in the congregations of Christ Church and St. Peters from Christmas, 1781, to Christmas, 1782, 24 died of the smallpox. In that year 12.1 per cent of the deaths in these two congregations were due to smallpox. The population of Philadelphia was then approximately 54,000. In the last three years, with a population forty times as large, no one died in Philadelphia from smallpox. The disease is unknown in Philadelphia today.

There are those in this audience who can recall as I do the time when practically every summer our newspapers carried alarming headlines announcing an outbreak of yellow fever in

Mobile or New Orleans or other Southern cities. There are those who can recall the precautions which were taken at the Port of Philadelphia to prevent the introduction of Asiatic cholera. How many of you remember the days when we spoke of "consumption" as an inevitably fatal disease? You may recall as I do the belief of mothers that their children had to run the gamut of children's diseases. The "second summer" was particularly dreaded. We awaited with such philosophic calm as we could maintain, the attacks of croup, diphtheria, chicken pox, measles, and whooping cough, as diseases which children undoubtedly would become prey to.

We believe differently today. It is the general impression that all the diseases I have enumerated are due to bacterial infection. Some of them are known to be preventable. For this reason we are convinced that there is no need for children to suffer from them. In one instance, diphtheria, the cause has been isolated and the means of prevention found. There is hardly a more illuminating page in the field of preventive medicine than the discovery of antitoxin by Behring in 1890 and the subsequent discovery of toxin-antitoxin. Since the use of the latter, diphtheria is rapidly disappearing. In New York State, excluding New York City, during the last five years the diphtheria mortality was reduced 75 per cent over the previous five year period. The diphtheria death rate is approximately a little over 2 per 100,000. A continuation of the intensive educational campaign to have children immunized will eventuate in the utter disappearance of this disease. What has been done in New York State can be done everywhere. Diphtheria will follow the paths of cholera and yellow fever and other diseases of infectious origin which today are rapidly diminishing in volume. What is true of diphtheria may be true in the next few decades of measles, whooping cough, and scarlet fever. We may look forward in confidence to the realization that children may reach adolescence, not only free of these diseases, but what is of equal importance, free of their damaging sequelae which frequently manifest themselves in adult life.

The picture, therefore, which we have today is the prospect of the disappearance eventually of most if not all diseases of germ origin. Newer researches will give us additional information regarding certain diseases whose causes have not as yet been definitely ascertained. It is one of the ironies of the situation that we still know comparatively little about the most common disease. I refer to the common cold. Here too there are indications, even more than indications, that we are on the verge of discovering its cause. When we shall have found it, we shall have made great progress and shall have relieved humanity of the most frequent cause of debilitating illness. Sleeping sickness, infantile paralysis, and pneumonia are in the same category. As yet research in these fields has not reached the point of completion so that we may know how to prevent them. But anyone who follows the research work now being done in many laboratories cannot help but feel that in these fields as well we shall soon know not only how to cure, but how to prevent.

What does all this mean so far as the expectation of human life is concerned? What have these marvelous discoveries done? The statistics of the past one hundred and thirty years amply tell the story. According to the life table which Mr. Barton presented to the American Philosophical Society, the expectation of life at birth in 1790 was approximately thirty-five years. Of 1000 individuals born in one year, approximately 40 per cent died before they were three years old, and only five lived to be eighty years of age. The sum of their ages was 35,000 years, or an average of thirty-five years per individual. The next decade, and those in the beginning of the nineteenth century, showed comparatively little progress. Between 1790 and 1890, a matter of one hundred years, there was an increase in the expectation of life of only eight years. In the following decades the discoveries of the bacterial origin of disease began to bear fruit. The results are truly illuminating. In the United States in the twenty years between 1890 and 1910, the expectation of life increased eight years, or as much as in the previous one

hundred years. From 1910 to 1930 there was another increase of eight years. In other words, in the four decades following the discovery of the bacterial cause of disease, the expectation of life increased twice as much as it had in the previous century.

What is the outlook for the future? It is hardly necessary to guess. Certain facts are self-evident. We shall year by year apply more extensively the knowledge we already have regarding diseases which are preventable. Science is still far in advance of the application of its discoveries. The application of existing knowledge universally would further reduce the incidence of disease. I may cite here the remarkable campaign against tuberculosis, the "consumption" of our boyhood days, the disease which was supposed to be incurable. In the last thirty years the tuberculosis death rate in the United States has been cut to a third. A death rate of about 200 per 100,000 in 1900 has been reduced to a death rate of about 70 per 100,000 in 1930. There is still much to be done, but there is every reason to hope that within the next few decades the death rate from this disease can be cut very much further. Many communities now have death rates as low as 30 per 100,000. This enviable condition will in the course of years be general.

This is true all along the line. Deaths in infancy undoubtedly can be reduced below the average of 70 per 1000 live births. We look forward confidently to an average infant mortality rate of less than 50 per 1000 births. Much more must still be done to reduce the deaths of infants in the first month of life. Many of these are due to congenital and other causes. When we have found these, we shall know how to prevent them, and infant mortality such as we know today will be a thing of the past.

Recent researches indicate that a common disease of childhood, rheumatic fever, is probably of infectious origin. So-called "growing pains" are probably due to bacteria and are pathologic, rather than physiologic. This, too, will bow to the wand of science. We shall know how to prevent this

disease and, what is of equal importance, we shall learn its relationship to diseases of the heart which manifest themselves in later life.

In this realization we can confidently look forward to a further increase in the expectation of life. Such hope is more than speculation. It has the exactness of a mathematical calculation. If we can further reduce the incidence of transmissible diseases, the expectation of life will increase from sixty years which it is at present, to seventy years. Whether the expectation of life can be extended beyond this age will depend altogether upon further researches in medical science.

You will note that I have said nothing until now of the "span of life," the main subject of this address. We must distinguish between expectation of life and span of life. The former is the average number of years which a group of individuals may attain. By span of life we mean the maximum number of years which an individual may attain. Mr. Barton dwelt upon the fact that in his day individuals attained eighty years of age. The history of vital statistics is replete with evidences of individuals who have lived longer. Men have lived to be a hundred and beyond. There are records of individuals living one hundred and fifty years. The authenticity of such cases is questionable. There can be no doubt that here and there men and women have attained ages far beyond the average. What likelihood is there that in the future the span of life may be increased? What likelihood is there that science may find the means of prolonging life far beyond the normal of today?

I am making a conservative statement when I say that there is no evidence at this moment that human life may be prolonged beyond its presumed biologic limits. All the evidence indicates that man, like other animals, lives an allotted time. All that has been done thus far has been to save the wastage of life at the younger ages. Many more individuals today live to be fifty, sixty, seventy, and eighty years of age than was the case a century or two ago. It still

remains, however, that after age ninety, there are comparatively few survivors and that those who reach even more advanced ages are even fewer.

It would be idle speculation at this time to predict that human life may be extended indefinitely. And yet, we must remember that man differs from other animals in that he is a reasoning being. In the animal world there has been the constant struggle for survival. Each genus of animal has developed a biological ability to live a prescribed time. Certain insects live a day. The elephant lives two hundred years. These animals have adapted themselves to their environment. Man differs, however, from all other animals. With man as a reasoning being, the future will be not merely "the struggle for existence," but as Lange so aptly put it years ago, "the struggle against the struggle for existence." Man has not only adapted himself to his environment but in many instances has conquered it. He has learned to protect himself against heat and cold and the elements. He has made inventions and discoveries in this fight for longer existence. Daily he is learning more of nature and daily is making nature his servant. How far this may go no one can tell.

Certain significant researches in the recent past should be mentioned. They are at least suggestive. Much is being done to ascertain the actual mechanism of the aging process. There are varying views and opinions. Child has concluded that senescence is not a phenomenon of later life but that the human organism begins to grow old almost from birth and that the aging process is most rapid in youth. Carrel believes that the number of years during which a man may live bears no definite relation to his real age. Any method for measuring old age must be based on certain physiological and chemical modifications which occur in the blood serum. Stockard agrees with Carrel that it is not age which affects cells, but a gradual change or modification in their surroundings which tends to lower their growth reactions.

Not only the cell is being studied. In recent years we

have learned much regarding the glands of internal secretion—the thyroids, and the adrenals, and their effects on the human organism. We have gone even further. Crile states that he may have created the first artificial living cell, which may be broken, mended, and reassume the activities of a living thing. Carrel has kept alive for twenty years cultures from individual chicken cells and suggests that in a suitable medium they might be kept alive indefinitely. Osterhout has been able to follow the process of death step by step by measuring alterations in protoplasmic potential and in electrical resistance. Pearl and MacArthur have studied the problems of metabolism and conclude that the duration of life varies inversely with the intensity of the metabolic processes. If McCollum is correct, rats lose their mother love when fed on a manganese-free diet. Who knows what the future may hold in store if the theory is correct that the universe and mankind are akin and that both are made up of protons, electrons, and photons.

Whether the span of human life can be extended is not a matter of a day. Research for many decades and in entirely new fields will be required. Any attempt at this time to predict what may happen would be idle.

But even if it should be shown that there is a definite biologic limit to human life, is this necessarily a cause for disquietude? Would much be gained if we knew we would live one hundred and fifty years or even two hundred years, if old age means not only senescence but too often senility? Is there much comfort in that picture of Shakespeare's, "Sans teeth, sans eyes, sans taste, sans everything"?

Is not the real problem which immediately confronts us, not the extension of human life but a further reduction of disabling and debilitating illness? Today 2 per cent of American wage-earners are constantly so ill as to be incapacitated for the ordinary pursuits of life. If I were asked to give a slogan for the next fifty years which would indicate the best efforts in life conservation, it would not be, "May you live one hundred and fifty years," but rather the slogan, "Cut the illness rate in two."

Life means comparatively little to the patient suffering from chronic disease. We still have our armies of the halt, the lame, and the blind. Hospitals, penitentiaries, and asylums are filled with living examples of the sequelae of childhood diseases or the inheritors of venereal disease. Many organic diseases of middle life—diabetes and certain forms of nephritis and heart disease—are not transmissible diseases, but may be the results of childhood infection. Whenever we can adequately protect the child against bacterial invasion, when we can give him a clean heredity, when we can teach him personal hygiene, and give him a proper mental attitude, freedom from care and worry, and opportunities for rest and recreation, we shall have entered that newer campaign which spells the postponement of disease and the promotion of physical, mental, and moral health.

Think what this may mean from the standpoint of human efficiency. Think what it may mean in a consideration of the future of this changing world. The past is a promise of what may happen. If I were a prophet I would try to vision the world of 1980. I think I could picture a changed world both in its economic and social aspects. It has been estimated that the savings in mortality due to the reduction in the death rates between ages twenty and sixty, particularly from diseases such as tuberculosis, the savings due to the maintenance of earning efficiency at these ages between 1901 and 1920, have meant a half million fewer orphans, of whom the majority would have been dependent by reason of the death of the wage-earner. The history of welfare work in the United States during the same period shows a marked reduction in dependency due to illness.

The picture of the future is clear. Orphan asylums will we hope become things of the past. There will be less need for welfare organizations for the indigent. Year by year we shall have fewer hospitals. The job of the doctor will more and more become the prevention of disease, rather than its cure. Industry, in view of our constantly increasing develop-

ment of technological processes, will be able to maintain efficient production with a marked reduction in the length of the working week. Because of the stabilized population resulting from the lower birth rate and the advancing age of the future population, the laborer at forty will not be looked at askance when he seeks work; nor will he be discharged at fifty to be replaced by a younger man with less skill and less experience. The maintenance of a larger number of men in industry as a result of this may change our industrial system. The world will have more leisure. The problem of the future will be the adequate use of leisure. Recreation, amusement, education, mental and spiritual improvement, which in the past were the privilege of the few will be the rights of all. It will be a far cry from the days preceding the industrial revolution when men worked at the handloom twelve to fourteen hours a day.

Our educational system will be altered. Health education will begin in the nursery and the kindergarten. Teaching health habits will be the function of the school and will be as important as the teaching of the three R's. Children will grow up with an understanding of the term, "vibrant health." The full activity of the adult will be employed to earn a livelihood in congenial work and to possess himself in his hours of leisure of those tremendous possibilities which the world offers and will continue to offer in increasing measure in physical, mental, and spiritual enjoyment.

In substance, unnecessary disease must disappear. Death must no longer come in childhood. Children must not be compelled to work before the completion of adolescence owing to the untimely death of parents. Debilitating, incapacitating illness must be postponed. Each of us should have the hope of growing old gracefully, in the possession of our mental and physical powers, so that whatever the biologic age, whether the present one or whether through the efforts of science it may be extended, the final break-up will come as came that of the deacon's one horse shay. A century of unimpaired usefulness and then dissolution, with springs and

axles and hubs and tires going to pieces all at once. When we can approximate this happy state, we shall understand what another great American poet meant when he asked us to meet the great finale "like one who wraps the drapery of his couch about him, and lies down to pleasant dreams."

TECHNOLOGY AND MATERIAL PROGRESS

By WILLIS R. WHITNEY

(*Read April 23, 1931*)

THERE is nothing in fixity. It is a figment of the imagination. The fact is old, the discovery is modern. Progress and not product dominates mankind. This has always been a changing universe. Perhaps the cave-man noted no change, but nevertheless its continuity had already been long established. Nowhere in the widest stretches of astronomical time is there any sign of one changeless period. The starry firmament itself has only been known to us through its changes. We interpret rays from the stars as proof that the most stable things on earth, our very elements, are being reduced and produced. The atoms are wireless stations which broadcast information of changes quite beyond our earlier comprehension.

Continuous change marks not only celestial systems and our inorganic worlds, but every living thing, from polyp to politician. All have developed by change and are still changing.

The single living cell which first slipped the hawser that anchored it to some submarine rock, hopefully wiggled its residual stump, or cilium, and, thus experimentally moving about, found it could better meet its food half way. Thus changes for good in life may be due to an unsatisfied, but not necessarily dissatisfied previous state. Being unsatisfied or inquisitive is not safety-first, and in the Devonian age many an experimenting fish must have died at low tide before satisfactory lungs were developed.

Certainly the persistence of change, the absence of fixity which is so evident today, must have been seen by early philosophers, though I suspect that the possibilities in change, nay, the certainty that changing, or progress, is the all-

important thing had not been discovered at the time of Socrates. I find him saying in Plato's "Republic" something which is not further expanded. In considering the possible source or training of leaders for the republic, Socrates asks briefly, "What sort of knowledge is there which would draw the soul from *becoming* to *being?*" I am supported by Jowett's notes on the Republic, which contain this significant reference: The regular *growth* of a state enlightened by *experience*, *progressing* in knowledge, improving in the arts of which the citizens were *educated* by the fulfillment of political duties, appears never to have come within the range of their hopes and aspirations."

Our knowledge of Nature's laws is always incomplete and ragged. Man-made laws are imperfect and inconclusive. The edges of all human fabrics are rough and frayed. Our inventions and devices are like little islands rising from an infinite ocean, or like living trees in fertile fields growing at all exposed surfaces. All sciences are adding new science at the tips of countless branches today, and must always do so. This fact in turn becomes a necessary and inseparable part of our technology and material progress.

Once the men of every race were less appreciative of their possibilities of change and more deferential to powers in lower animals. Every country, every tribe, and almost every family at one time adopted, and even worshipped, some lower animal. So we have the American Eagle, the Russian Bear, the British Lion, the Mohawk Turtle, the Egyptian Scarab, the Chinese Dragon, etc. Francis Bacon attributed to Herodotus the view that the Egyptians deified many animals because they were so much better discoverers and inventors than men. It is as though inquisitive change was recognized in lower animals long before man saw any direct possibilities in discovery for himself.

In some respects, the beginning of advancement of science might be attributed to the 12th and 13th centuries. At that time (or even earlier), foreign students in European cities, living abroad to acquire new knowledge in medicine, religion,

civil customs, etc., were forced to protect themselves by forming organizations which became the universities. This process of international spread of knowledge has never been reduced, and some of our modern physical, chemical and mechanical processes and products are directly traced to researches in pure science, carried out entirely under university auspices. In other words, it was through the efforts of ancient organizations, themselves changing or progressing, that the scientific foundations were laid for our material as well as our spiritual progress. The process has become continuous and at no time has there been absence of a spiritual aim.

Having the possibilities of infinite change in mind, Bacon in 1600 wrote powerfully against the inactivity of men who were limited by fear and superstition, taboos and cumbersome words.

A change did, indeed, begin about this time. Men were encouraged to seek freedom from false gods, from mysterious words, from ancient traditions; unprejudiced attack by experiment and observation was suggested, and means for improved communication, for economical recording and preserving of truth were devised. Cheap printing was in vogue.

From then on even those institutions of highest religious aim turned gradually toward considering the lily, and to diligently questioning and enjoying study of the rest of the universe. Galileo and the first scientific society date about 1600. The British Royal Society (1662), the French Academy of Science (1666), the Berlin Academy (1700), and the American Philosophical Society (1769) followed this great change. Gradually the universities all over the world began devoting effort to progress in new knowledge. Not satisfied to be mere preservers or storehouses of collected wisdom, they learned by experiment and saw that there never need be a limit to advancement of knowledge. Its acquirement by direct attack soon took place in countless different directions, and in all civilized countries, and the whole orderly product received the title of "Science." It is having such a broad

influence that we need not expect man to move in cycles or circles. He is progressing rather in an ascending helix.

Without delving deeply into technology and material progress, I wish to introduce a few specific instances of this early perception. I have enjoyed noting the various ways in which men first commenced to express faith in progress. No other animal does it.

I like to think of pious old John Woolman, who would not allow his clothing business to expand lest it interfere with his spiritual growth, as being appreciative of progress. He believed it possible to "provide all men with an environment which will best develop their physical, mental and spiritual powers." This was not mere theory with him, for he sought to apply it when he helped prepare the way for changed treatment of the American Indians, and, over a century before the Civil War, fought earnestly for the freedom of the slaves.

Baron de Tocqueville, writing of the Americans in 1850, said: "They have all a lively faith in the perfectability of man; they judge that the diffusion of knowledge must necessarily be advantageous and the consequences of ignorance fatal. They all consider society as a body in a state of *improvement*, humanity as a changing scene in which nothing is or ought to be permanent, and they admit that what appears to them today to be good, may be superseded by something better tomorrow." He adds, cautiously, "I do not give all these opinions as true, but as American opinions."

Bearing on de Tocqueville's remarks, Mr. M. E. Tracy recently wrote in the World Telegram: "To a great extent, we Americans have cultivated an insatiable thirst for change and innovation. We want nothing so badly as new methods and new devices. We are intrigued by nothing more distinctly than the thought that there is bound to be something different just around the corner. The appetite for experiment, discovery and invention is in our blood."

Modern philosophers, like Bergson and John Dewey, have advanced about as far as people are yet willing to follow in

this view of progress. It is a bit novel to think of the process of change as more important than any finished product. We have naturally a thought of the importance of arrival, the imminence of the millenium. But arrivals are only rising steps of immortal growth where the worth while thing is climbing, not resting. Bergson, in Creative Evolution, says, "We change without ceasing. To exist is to change, to change is to mature. Duration means invention, the creation of forms, the continual elaboration of the absolutely new." There is no sign of fixed states here.

John Dewey has said, "The vanity and irresponsibility of values that are merely *final* and not also, in turn, means to the enrichment of other occupations of life ought to be obvious." The processes of growth, of improvement and progress rather than the static outcome and result become the significant thing. "Growth itself is the only moral end." "Not perfection as a final goal, but the ever enduring process of perfecting, maturing, refining, is the aim of living."

In 1895 Professor William James heard a Harvard teacher say, "All the fundamental conceptions of truth have been found by science, and the future has only the details of the picture to fill in." Professor Wilhelm Ostwald had just expressed the same thought in Leipzig. James vigorously denied this theme and said truly, "Our science is a drop, our ignorance a sea." Since 1895, radium and the X-ray have been developed, the atom broken down, the electron discovered, Einstein's generalizations produced, the quantum conception provided. Our bones are now made visible, we communicate with Europe by radio, television is in sight, aeroplanes have become common, and we admit that we know less about the essence of time, space, gravitation, and light than was known in '95. Truly our ignorance is a sea, our knowledge almost an evaporating drop. But the fortunate thing is that we are still changing.

We see new industrial experiments carried on all around us, but do we realize that they constitute progress, and that this is more than ever possible through the magnitude of the

experiments and the facility with which they are made public? From the remote concentration of human physical efforts in Russia and the unprecedented trials in England's dole, to our own internal novelties in the way of unemployment relief and veterans' bonus loans, the world is trying changes. And it is too early to compare with certainty the effect of the underpayment of the one with that of the overpayment of the others.

Our international technical possibilities are like the sinews of the child, not easily broken, but not yet tested or developed. We use radio for mere amusement and noisy advertising, our wealth, for armies and schemes for destroying our neighbors. We cannot change at once, but we realize that there is a gradual tendency to get together and to live in peace.

Count Keyserling, in his Paris lectures on the domination of the machine age, looks at our present civilization in the United States as the "tragic misconception of the modern epoch traceable to a failure to recognize that man is essentially spiritual." He may be right. But all former civilizations were still more tragic misconceptions, if knowledge and truth are criteria. There has never before been a time when a man, speaking on the banks of the Seine, was heard in the reaches of the Trocadero through mechanical amplifiers, and his words published all over the world on the following morning. In fact, if the spiritual leadership of such a man were evident even to a very small number of his fellows, his voice could be instantly broadcast to the world by devices which mark, as plainly as anything does, our machine age.

The speed with which we are applying new knowledge seems dangerously rapid to some, but there is every indication that it will not be reduced. An individual, or a nation, may decide that it has experienced a too rapid mechanical progress, but, as long as others advance, there will be an increasing tendency to bring all people to whatever has apparently (at least temporarily) proved to be the most satisfactory condition. In other words, it has always been, and probably will always be, a changing scene, with new experiments pointing a way to better conditions.

There never was a time when so many people in one nation, or so many nations of the world, were trying to advance. There never was a time when technical and material progress was more constructively attempted and critically examined. There never was a time when anyone's efforts for good were so quickly and so generally broadcast. There never was a time when youth was more earnest or fearless in seeking the essentials of truth. The accumulated data of all material progress never were so great and never so uniformly appreciated. If one country slackens in gaining new knowledge, the whole world knows it at once. If another country, or even any individual in it, advances the science of some particular field but a trifle, the rest of the world begins at once to use it. Pavloff's experiments in Leningrad on salivating dogs are quickly coördinated with psychological researches in America, and these in turn with brain mechanics, and then operators in highly mechanized manufacturing plants are experimentally chosen, graded, or discharged according to reflexes and psychological reactions.

Those who are interested in technical progress look at it as continuous, but do not necessarily overrate its importance. There must be a parallel advance for the higher values in man. Perhaps the best way to look at our materialism is just as we now look at its earliest examples, for we are but a very short way from what may be called our real beginning as thinkers.

All the early discoveries which first insured bare preservation through continued effort, were augmented by technical discoveries like tool making, food growing, fire building, and animal control. These in turn were followed by time-saving and time-integrating developments like writing and printing. Our present accessories in electricity, mechanics and electronics, important because of proximity, are only the latest added steps, not the last. They lead to new kinds of people with new kinds of minds. This is what man at every previous stage has devoutly sought for, earnestly fought for, and generally acquired.

There are errors in scientific conclusions now, just as there

have been in the past. Hardly a single scientific fact of one century remains adequate for the next. First the world is flat and the sun rises; then the world is round and the world goes round the sun. Then the whole system moves through infinite space towards Alpha Centauri, and then the space loses its infinite quality and adds a curvature. I don't expect to see the end of changes, nor will anyone else, because the last man will insist on making them while he improves. Our conceptions, discoveries and uses of an unfathomable universe are certainly always flexible and subject to improvement.

I think the world is more anxious to go right than ever. It is more eager to develop intelligently and not stop at some temporarily agreeable state. It is learning that any conceivable fixed state is not worth while so long as we still possess the power to advance.

It is futile to expect a world which is already enlightened to the advantages of material knowledge, mechanical substitutes for physical labor, and the promise of freedom for better growth in the future, to reduce its efforts or change its direction.

Man *is* essentially spiritual, but his tokens of values, his media of exchange, the flowers of goodwill to others, call for material (even mechanical) devices. The Greek slave, the Egyptian fellah, and the man-with-the-hoe developed into the modern, less-enslaved philosopher who sees that man is essentially spiritual. If there is one thing modern mechanical civilization can do, it is to free people from slavery and strew spiritual opportunity along their path.

Section II. Tendencies in the Field of the Social Sciences

ECONOMIC ADJUSTMENT IN A MACHINE AGE

By ERNEST MINOR PATTERSON

(Read April 23, 1931)

In the middle or latter part of the eighteenth century there began the transformation of economic life known as the Industrial Revolution. About one hundred years later there came a movement which may be thought of either as new or as an acceleration of the earlier one, since in some of its characteristics it is similar. During the last few years it has often been referred to as the Second Industrial Revolution. As it proceeds, both students and the general public are becoming conscious that a new set of problems are being presented. Because of its widespread influence readjustments must be made with increasing rapidity. Both our thought and our conduct are profoundly affected.

This paper is limited to a discussion of the economic significance of these changes. Of course such a limitation is an arbitrary one. All aspects of life are affected. Nevertheless, the economic consequences are among the most immediate and the most important. Machines are devices by which other than human and animal effort is utilized in the processes of production and distribution. If such a transformation is controlled and if its more obvious benefits are to be attained these machines will lighten human toil. The world will have more commodities available with the expenditure of the same amount of time and effort or perhaps will content itself with the same number or a slightly increased number of commodities and take all or a part of the gains in the form of leisure.

Some Characteristics of the Machine Age

Among the economic characteristics that might be mentioned four are chosen for brief emphasis. First is the fact that the word "machine" is for the present purpose too narrow in its connotation. New and more intricate mechanical devices are constantly appearing and are a part of the phenomenon, but our current problems are intensified by the fact that today, as never before, science is being utilized in economic affairs. All fields of knowledge are drawn upon. Physics, of course, continues to contribute, as in the past. To an increasing degree chemistry is an aid, as, for example, in the dye industry. Not only botany but, more and more each year, biology and psychology are being utilized. Thus the psychologists are called upon to assist in the adjustment of the worker to the job and in the relations between employer and employee. Mathematics, as the basis of statistics, and in many other ways, aids in the investigation and in the interpretation of economic data. Perhaps more than ever before ethical considerations are powerful in economic life while an increasing number of economists are seeking aid from modern philosophy.

This brief statement is only a hint of what is occurring. Scholars are familiar enough with the disappearance of the barriers between fields of knowledge but perhaps not all of us are aware of its economic significance. Our second industrial revolution is profoundly affected. Changes are not merely in the substitution of water power, steam and electricity for human or animal effort, but in the application of all human knowledge. No simple word is adequate to describe what is occurring. In Germany and to some extent elsewhere the term "rationalization" is employed. Its definition is not easy because it is so comprehensive, but in any case it falls far short.

A second characteristic is that changes are appearing with increasing rapidity. No business man can feel sure that his new machine or tool or formula or method will for very long be the latest word. Without warning his plant becomes out

of date. The most modern apartment house or office building is soon antiquated and must be replaced or at least yield a reduced income because something so much better has been erected. Trucks suddenly become disastrous competitors of the railroads. The cinema threatens the legitimate theater and the livelihood of many actors, but provides employment for large numbers of musicians. Almost over night the "talkie" is perfected. Actors with poor stage voices are dismissed and others with good voices are once more in demand while the unfortunate musicians have nothing to do but to carry on a futile advertising campaign against "canned music."

As a corollary of this the costs of distribution rise. As transportation and communication are rapidly improved goods are transported greater distances. The British consume dairy products from Denmark, fruit from South Africa and from Canada, eggs from China, and fowl from the American northwest. This reacts to the injury of the British farmer. Also it may be noticed that transportation costs and the costs of advertising and selling become a larger and larger percentage of the total. In fact many observers see these items of expense growing so rapidly as to offset or perhaps more than offset the economies that are being introduced in the primary processes of agriculture and in manufacturing.

Third to be mentioned is that these economic changes are appearing more rapidly in some countries than in others. The resulting strain is enormous. After 1870 English economic life suffered a relative decline while Germany and the United States forged ahead. During the late World War still other countries industrialized so extensively that England was placed at a further disadvantage. When the Russian revolution in 1917 brought a socialist regime into power its speedy downfall was prophesied. Today as its five year plan develops the industrialists of other countries profess alarm, declaring that they cannot compete with this new type of organization. Their fears may prove groundless but in so far as they are

warranted we have another illustration of the irregularity with which modern economic changes are occurring. Large industries in a highly prosperous condition are suddenly imperilled by the appearance of competition in a distant part of the globe.

Many other characteristics might be added, but only one more can be mentioned. There are many lines of production in which the small scale producer remains dominant. On the other hand, there are many whose units are constantly increasing in size. As the size of plants grows and as consolidations take place the overhead costs become a larger fraction of the total. A given enterprise such as a steel plant or a sugar refinery or a railroad must make heavy expenditures for interest, insurance, maintenance, obsolescence, taxes and what not, even though operations stop entirely. These overhead costs may be 60 to 80 per cent of the total.

This results in fierce competition. Since so much must be spent even though no orders are received, each new order becomes important. Advertising is enlarged, selling efforts are intensified. Additional business must be done whatever follows. Markets at home and abroad must be secured no matter what the sacrifice. At home law and regulatory bodies and business codes are restraining influences, but in the international markets, if competition prevails, the methods will be ruthless. And as the costs of competition mount the inducements to coöperation or combination grow.

Some of the Results

These are a few of the outstanding characteristics and they suggest the problems to be met. Perhaps Americans even more than Europeans are conscious that economic life is now organized largely on a world basis. This is not new except in degree, but a change in degree may be highly important. For many centuries the people in every part of the world have bought and sold in many other parts. Today each of us buys more foreign goods than ever before and sells more of

his own product abroad than his ancestors ever did or than he himself did only a few years ago. Probably this practice will grow rather than diminish.

Moreover, these economic contacts affect us more than those of the past. A few centuries ago world trade was largely in the luxuries of the time. Today we buy and sell the necessities of life in all parts of the earth. The British import perhaps two-thirds of the food they consume and if these foreign supplies were cut off millions of the inhabitants of the United Kingdom would soon perish. Only to a somewhat less degree do many other people depend on outside sources for food. Raw materials of the most fundamental kinds must be imported in a steady stream and the goods manufactured from them regularly exported. If the movement is retarded disaster follows.

Attempts to lessen this world wide dependence have been ineffective. Germany preferred to develop her own manufacturing industries and utilized protective tariffs and other devices for the purpose. The result has been a growing need for food from abroad, an increased demand for cotton, copper, and other raw materials, and an extensive dependence on foreign markets for her manufactured goods. She developed a merchant marine in order to be independent of foreign merchant ships and at once became dependent on carrying freight for others to keep her own ships busy.

Italy too desired more economic independence and developed an industrial life. Today that life depends on British and German coal, on American cotton, on foreign commodities of every sort and description. Russia is attempting a stupendous development but is entirely unable to cut herself off from outside help. She is importing tools, machinery and technical advisers and is exporting everything she can spare from domestic consumption. Even the United States is being drawn more and more completely into the complications of international economic life through her trade, her ships and her investments.

A second consequence is that competition is greatly

intensified and more dangerous. With competing units larger and more intricate, with bondholders and stockholders clamoring for interest and dividends, but caring little for the business methods employed to secure them, with overhead costs as an ever present urge, business management must find markets. Prices must be kept as low as or lower than those of a competitor and hence costs must be reduced, particularly when the price level is declining as it has been during the past two years. Since wages bulk large in costs many employers will attempt wage reductions. If wages hold firm machinery may displace labor and technological unemployment will increase. If prices are rising, there will be a corresponding competition for raw materials.

In the domestic field this intense struggle may be, and often is, both wasteful and brutal. Petroleum production in the United States illustrates what may occur. Reserves are drawn upon, and prices are lowered to a point of demoralization but without reducing consumption. Our natural resources in oil are diminished, production is inefficient, price wars demoralize the trade with only a temporary and somewhat dubious gain to the consumer. This disgraceful development is hard to check because of our common and statute law with its prohibition of "combinations and conspiracies in restraint of trade." In spite of the desire of producers to coöperate in some manner and reduce losses our inability to adjust our legal structure to modern conditions has thus far prevented the application of any appropriate remedy.

Another illustration may be helpful. The sugar industry is one of the most demoralized in the world. Cuba and the Dutch East Indies are the largest producers of cane sugar while sugar beets are raised in many areas including Germany, Czechoslovakia, Poland, Belgium and Hungary. The interests of these countries are attempting to curb wasteful competition in the production and marketing of this product. Progress is being made with the ultimate results far from certain. But at this very time when coöperation is urgently needed for the sake of millions of people whose lives are

demoralized our Department of Justice feels compelled to bring action against our sugar interests for violation of the Sherman Anti-Trust Law. This is not a criticism of the Attorney-General, who may have no choice in the matter, but it seems to be an illustration of one of the unfortunate results that are coming and of the need for legal adjustments to meet changing conditions.

Under such circumstances attempts at coöperation through combinations, consolidations, cartels, working agreements and in other ways may, of course, be expected. Often these will be due at least immediately to the desire for profits, but this need not blind us to the social significance of the movement and in many cases to its desirability. Opposition will for the most part be about as effective as the famous efforts of King Canute and Madame Partington to restrain the sea.

The most serious aspect of this development is the tendency toward business organization along national lines and with governmental coöperation. A few illustrations, briefly stated, must suffice. Many American industries through the device of the holding company or otherwise have a high degree of monopoly within our own borders. More important for the topic in hand is the extent of coöperation between the Federal Government and business to further American trade in all parts of the globe. We even have legislation that aids, in the form of the Webb Act and the Edge Act.

But what we are doing is still less portentous than movements elsewhere. The British Government was actively helpful in supporting the Stevenson Plan for steadying the price of rubber; the Brazilian Government has for years had its coffee valorization arrangements; and much similar assistance has been given in other countries. The British Government has recently set up an economic advisory committee comparable, according to Prime Minister MacDonald, to a military board of strategy. Similar, though not identical, methods may be found in Germany, Italy and Czechoslovakia.

Of course Russia is at present the most prominent illustra-

tion. There has been much foolish hysteria about the five-year plan and about Russian dumping. Yet the Russian method has much in it that is suggestive and deserves brief comment. Dumping is to be explained by the fact that a producer, whose home market is protected by import tariffs or otherwise, may often sell abroad at a very low price, keeping his total receipts up through higher prices in the domestic market. A large concern or group with many by-products finds this method contains vast possibilities for gain, although it may bring demoralization to rivals in other countries.

In the case of Russia we have an entire country organized under one directing leadership which has a high degree of control over both domestic and foreign trading, not merely in one industry but in all. Low prices in one market or for one product may be made up in other directions. Without exaggerating at all the current situation or forecasting the outcome of the Russian experiment, we may emphasize the possibilities under such an organization.

Russia is mentioned because it is the outstanding illustration of the way in which national economies tend under modern economic conditions to organize themselves in opposition to other national economies similarly organized. Many other countries are moving in the same general direction. We might add that another form of a somewhat similar tendency is to be found in the development of customs unions and the proposed economic United States of Europe.

A third consequence of modern developments is the necessity for rapid economic adjustments. As Mr. Elmo Colkins says, "Business has wings." Reference has been made to the rapid changes in building construction and in the moving picture industry. Illustrations might be multiplied. Accountants express the problem by telling us that they do not know how to write off obsolescence. Manufacturers worry over the competition of rival products and competitors' methods. Students of labor conditions are disturbed at the amount of technological unemployment.

Finally, we may mention a growing difficulty in maintaining adequate economic and social stability. With other countries imitating and improving upon their industrial methods, the English find their economic and to a degree their political regime upset. In the last twelve months rapidly falling price levels have had much to do with Latin American revolutions. Rapid developments in other parts of the world are forcing our agricultural groups into defensive tactics and ought to force them into intelligent long range planning instead of into blind economic warfare.

An outstanding expression of this consequence is the rapid growth of protective tariffs in recent years. These tariffs are in part due to the desire of the affected groups to increase their gains, and in part due to the setting up of many new governments in this post war period. But they are also due to the need of each economic group to secure at least a measure of stability and protection in a world whose quick changes are demanding readjustments more rapidly than they can be made. To the extent that this explanation is correct tariffs cannot be exorcised. There is no form of incantation that will be effective against them even though the effort appears as a resolution from a world economic gathering like that of 1927.

Some Economic Needs

In conclusion two observations may be made. One is that we have a growing need for intelligent anticipation of the future. There are those who are skeptical over the successes of business forecasting. There are even some who content that such attempts to peer into the future are logically impossible. Yet there is no doubt that our need is increasing. Professor A. N. Whitehead forcefully reminds us that life is largely a routine whose advantages must not be minimized, but that change is a fact gaining in significance. Intelligent anticipation is more important in economic life than ever before.

A second observation is of the need for rational planning: In 1776 Adam Smith could summarize his position by saying:

"All systems either of preference or of restraint being thus completely taken away, the obvious and simple system of natural liberty establishes itself of its own accord."

And elsewhere:

"Every individual is continually exerting himself to find out the most advantageous employment for whatever capital he can command. It is his own advantage, indeed, and not that of the society, which he has in view. But the study of his own advantage, naturally, or rather necessarily, leads him to prefer that employment which is most advantageous to society. . . . He intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention."

Such days are gone. No longer will the invisible hand guide us. Manufacturers have always shown their distrust of it by demanding tariffs and subsidies. Transportation lines rely upon favorable mail contracts. Of course we have never believed in complete laissez-faire.

Today, more direction is imperative. Plans must be thought out, methods of execution devised and guidance furnished. This may be by the state or by one of its agencies. Instead it may be by private groups. The organization and the method will vary from country to country and from time to time. Moreover, we have already passed into the era where mere national planning is inadequate. To discuss and to plan only for national prosperity may be futile or disastrous. We must study more and more what makes the world prosperous and seek to secure coöperation throughout the entire world. The task is stupendous but the alternatives to success are horrible to contemplate.

COMMUNICATION AND WORLD PEACE

By WALDEMAR KAEMPFFERT

(*Read April 23, 1931*)

When we communicate we are concerned primarily with the transmission and reception of ideas. No idea can be communicated without signalling. When we talk to each other we speak in sound signals, arrange them into words and sentences, and understand them because we have agreed on a system of interpretation. The invention of signalling or language is perhaps the most astounding of human achievements. And probably the most astounding of all signalling systems is writing.

Even in ancient times the transportation of a written message physically through space was so slow that flashes of light, puffs of smoke, and beacon fires were adopted. They were the equivalents of our telegraph. For lack of a code as elaborate as that devised by Morse they had but a limited sphere of usefulness. An army might be summoned to ward off attack, or the tidings of a victory might thus be spread abroad. When detailed communication was necessary there was nothing for it but to write out a message and send it physically through space by a runner. It took Julius Caesar twenty-six days to send a letter from Britain to his dear friend Cicero in Rome, and this by the best courier in his army. The method is not yet obsolete. Probably it never will be. Our postal service is still dependent on messengers. For all our fast mail trains and postal airplanes the bulk of the world's long-distance communication is still conducted by writing messages and transporting them by the swiftest available vehicle to their destination.

In the diffusion of culture, that is the communication of ideas, the invention of printing was clearly of paramount importance. Here we had a method of mass communication,

something remotely akin to broadcasting—remotely because the human transmitters and receivers had to know how to read and write. When historians came to realize that cultural forces are often of more importance than military exploits in shaping the destinies of peoples they even divided time into two eras—the pre-printing and the post-printing. Certainly the rise of democracy is closely related to the introduction and development of printing. Yet it would be a mistake to exaggerate the influence of Gutenberg and his followers on the world. The evolution of science and the introduction of mechanical and electrical energy were just as important. On the other hand it is hard to see how knowledge of scientific and engineering discoveries could have been spread abroad rapidly without printed descriptions and drawings.

When Niepce, Daguerre, Fox Talbot and their successors gave us photography, out of which have come first the silent motion-picture and later the talking film, another step in the diffusion of culture was taken. Writing and printing are but imperfect representations of speech, especially imperfect in English because many of our words are not even symbolically written as they are pronounced. The photograph does better, and especially the motion-picture because it reproduces the time element—gives us not a static moment but a sequence of events.

Probably no book ever printed ever had a circulation equal to that of a popular film play or so important a news reel as that which showed our troops marching under the Arc de Triomphe in Paris after they had landed on French soil to take part in the great war. Even in these days of the "talkies" positives from the same film negative are thrown on the screens of every country where there is a projecting machine. Some Congo tribes are as familiar with Charlie Chaplin's antics as are the inhabitants of any American city.

Such considerations as these lead Professor Shotwell to remark in his Introduction to the History of History:

"The mention of the moving-picture suggests that, if the test for the distinction between pre-history and history is the use of writing, we may be at another boundary-mark today. Writing, after all, is but a poor makeshift. When one compares the best of writings with what they attempt to record, one sees that this instrument of ours for the reproduction of reality is almost paleolithic in its crudity. It loses even the color and tone of living speech, as speech, in turn, reproduces but part of the psychic and physical complex with which it deals. We can at best sort out a few facts from the moving mass of events and dress them up in the imperfections of our rhetoric, to survive in the fading simulacra in the busy forum of the world. Some day the media in which we work today to preserve the past will be seen in all their inaccuracy and crudity when new implements for mirroring thought, expression and movement will have been acquired. Then, we, too, may be numbered among the prehistoric."

Here Professor Shotwell has placed an unerring finger on the defects of past methods of communication, particularly the limitations of writing and printing, and by implication indicated the ideal toward which the engineer and inventor is perhaps unconsciously striving. We want "the psychic and physical complex" of which he speaks. We want, in a word, the illusion of reality, of hearing, seeing, and feeling or of experiencing events. So long as we are dependent on written or telegraphic codes we fall far short of that ideal.

Photography was the first means of communication that faintly created the illusion of reality. Even the most detailed painting of a careful Dutch master could not record as much as a photograph made with a pinhole camera. Moreover the artist no matter how objective he tries to be is irrepressible. The man cannot help but inject himself into his representation of the outer world. With the motion-picture we place events where they belong in a limited time. The achievement of reality is startling.

In the motion-picture film we deal with one sense. To that extent the illusion of reality is not perfect. The telephone made it possible to dispense with the Morse code and convey ideas, by the spoken word. The illusion of reality is more perfect than that achieved by the motion picture. We

know that what we see on the screen is but an animated smear, a two-dimensional arrangement. We obligingly supply the third dimension in imagination. The telephone makes no such demands on the ear, despite the distortion to which speech is subject. What we hear is not a real voice but an electrical reproduction. So perfect is the reproduction that only one with some knowledge of telephone engineering realizes in the course of a conversation that a voice has been converted into electric impulses and the impulses transformed into acoustic vibrations that most of us accept as originals.

All electrical means of communication are but extensions of the human faculties through space. When we telephone from New York to San Francisco we send our lips, mouth, larynx, vocal chords, our whole talking apparatus across the continent in a sense. We talk into an ear in San Francisco, although we ourselves are in New York. The telephone was the first invention that extended part of the human personality almost infinitely into space.

And now the inventor has taken the next step. A primitive beginning has been made in television—in transmitting images of objects over wires and by radio from place to place. To be sure the images are two-dimensional, coarse-grained and small; yet they are recognizable patches of light and shade. Again something little short of a miracle has been performed. The face that one sees on the screen is a discarnate human being. The original of that face has been optically sliced and chopped into tens of thousands of points of light and shade, and these points, falling in proper sequence on a photoelectric cell, have been converted into electric impulses or waves. At the receiving station the electric waves are reconverted into patches of light and shade which are placed in their proper relative positions, the whole process occurring with such rapidity that the eye, unable to follow it, is tricked into accepting an ever-changing mosaic of light and shade as a whole.

Three years ago I undertook to do a little prediction after

the manner of Jules Verne and H. G. Wells—a prediction as to the future of electrical communication. I am glad to see that General Harbord, president of the Radio Corporation about two weeks ago published similar views. Let me summarize that future here because of its possible effect on the peace of the world.

The time is undoubtedly coming, perhaps within fifty years, when the twelve directors of a corporation in twelve widely separated parts of the country will hold a meeting in the New York office of the chairman of the board without leaving their desks. Their opinions and votes rather than their physical presence is required. But inasmuch as there are always opposing forces on every board the soft-spoken word may not be enough. Like poker players directors insist on seeing one another. So, a meeting two decades hence may well be a meeting of electrically disembodied personalities. The chairman sits in the usually very dignified, funereally upholstered room at the usual flat-top mahogany or walnut desk graced by the usual framed photograph of the usual wife and children. At the far end of the room are twelve television screens. In the office of each of the other directors are twelve similar screens. In thirteen different offices twelve voices and images, twelve electrical ghosts confer with a thirteenth. Each of the thirteen sees twelve faces before him. He talks to Stewart McDobbin just as if McDobbin were physically present and listens to McDobbin's objection to reducing the dividend from 6 to 5 per cent. And he knows that McDobbin is watching him and following his argument in favor of the increase just as closely. Documents are held up before the televisor by the chairman. They are critically examined and commented upon. It is even possible to sign telephotographic duplicates electrically by means of the telautograph, which has long been part of the equipment of every first-class hotel for the transmission of facsimile messages from floor to floor.

The spectacle of the officials of the Federal Reserve Bank, the Deutsche Bank, the Banque de France, and the Bank of

England traveling thousands of miles to meet in London or New York will pass with other quaint and cumbrous customs of the early twentieth century. Doubtless some future assembly of the League of Nations will confer without the tedious necessity of meeting at Geneva, though some of the delegates are Chinese and Argentinians. Even capitols and houses of parliament may be reduced to picturesque relics of a past when legislatures were compelled to meet in a given place. Filibustering will hold no terrors. Bored members will simply shut off televisors and telephones and go to bed.

Some day the images of television will be as large as those that smile and dance on the screen of a motion-picture theater. They will appear in all the colors of nature. They will lack a third dimension—solidity. Even that may be imparted to them if stereoscopic fidelity proves to be essential, so that instead of a two-dimensional reproduction, a recognizable, animated blotch on a surface, we shall see something that seems to have thickness, depth, density. That something will be as real as if we saw the three-dimensional, solid original through a window at the transmitting station. Yet we seek to touch it in vain. Our fingers encounter only the glowing screen on which the image dances. It is as if we sought to feel a ghost, a mere wraith.

But are we sure that the ingenuity of the engineer will not devise a way of enabling us electrically to overcome this difficulty and actually touch a hand a thousand miles away or to feel silk in all its smoothness and softness? It staggers the imagination, this possibility of transmitting and receiving touch through space. Yet is it any more staggering than television?

It is difficult to conceive the form that an electrical touch-transmitter will assume; but conception is the very essence of invention. There must clearly be a feeler of some kind at the transmitting station—something that will explore and palpate every millimeter of a face or a piece of fabric. Minute, almost infinitesimal differences in electrical resistance will be transmitted as the electrical feeler, artificial sense

organ if ever there was one, passes over a surface. But how these impulses will be reassembled at the receiving station to counterfeit texture, hardness, moistness, dryness, brittleness—that, for the moment, is more than an ordinary imagination can divine. Perhaps we may sit in an electromagnetic field in which free electrons are made to combine and separate and produce the electrical equivalent of hardness or softness. As we move the hand hither and thither we shall have the sensation of feeling a fabric or clutching the handle of a tool. Yet we shall feel and clutch only emptiness. Just as in television we see an image without substance, so in this emptiness we shall have solidity without body—what may be paradoxically termed abstract solidity. We shall be scientifically deceived, as we are when we hear a voice over the telephone or follow the images on the screen of a motion-picture projector or a televisor. Another human faculty will have been disembodied and sent through space. So we are confronted with telephoning and televising as realities and with teletacting as a very remote possibility. With these three we can go far toward reincarnating electrically disembodied personalities. Probably the reincarnation will never be wholly satisfactory, for the simple reason that it is easier to fool one sense at a time than two. I am tricked into accepting the voice that I hear welling from a radio loudspeaker as a real voice, even though I know that the effect is produced by a diaphragm that beats the air electromechanically, but no one has yet succeeded in convincing me that the voice that accompanies a motion picture, however accurately the two are synchronized, comes from the two-dimensional lips of the photographic singer on the screen. When I add teletaction to telephony and television I complicate still further the task of achieving the illusion of rality.

Immortality of a kind is assured to these electrical discarnations. Electric waves can be recorded in half a dozen different ways and reproduced. We may convert them into light waves and preserve them photographically; we may record them electromagnetically on a steel wire; we may

transform them into sound and embalm them on a phonograph disk. In one of these ways we shall preserve voices, images, as well as the grasp of a hand, the twitching of a muscle. Sound records and motion-picture films have already handed down to our day some phases of the disembodied personalities of artists who died twenty years ago. When teletaction is introduced scientific immortality of a kind will be an accomplished fact.

Now that we have sketched the past and future of communication we naturally ask questions about the social effect of writing, printing, the postal service, telegraphy, telephony, radio and television. Distance is now less important than time. It took about three weeks to go around the known world in the days of Pericles, the then world being confined largely to the Mediterranean countries. It takes about three weeks to circumnavigate the world today. Chesterton would argue that we have made no progress at all, despite our fast trains and steamers. As the world has been enlarged by explorers until it became a globe with a circumference of 24,000 miles it shrank with each new step in communication. Europe is but five days from us by the fastest steamer, twenty-six hours by airplane, a few seconds by telegraph and telephone. With the whole nation listening to the broadcast voice of the President of the United States, San Francisco and New York become electrical neighbors. Only the other day the world listened to the admonitions of the Pope. Never before had a pontiff spoken with such directness to so vast a multitude. The Pope ceased to be an almost symbolic figure. For a brief hour he became a vibrating, living reality. He seemed to have gathered the civilized world into a single colossal auditorium and talked to it with a voice so penetrating that it reached every corner.

The older forms of communication may not be so direct, but their influence has none the less been widespread. There can be no question that in the diffusion of culture the printing press has been thus far the most potent agent ever placed in the hands of man. It has been both a recorder and a scatterer

of ideas. Today, with the telegraph and the telephone feeding the newspaper, the outbreak of a revolution in Spain or the pregnant utterance of a powerful banker becomes known to millions in twenty-four hours. Add to this the dissemination of motion-pictures throughout the world, with their revelations of the life and the customs of alien peoples, and it would seem to follow that the old differences of opinion that bred war must give way to tolerance and peace.

There can be no doubt that the printing press, the motion picture, and latterly radio have gone far toward standardizing the world—perhaps too far. The clothes of a western European and an American are scarcely distinguishable in cut. From the Rue de la Paix come the modes for women. American efficiency methods are making their way in Europe. What we behold is a general leveling of culture. Genius is as rare as it ever was, but the nations of the world exchange cultural ideas more freely than ever before.

We hear it said, frequently enough, that all this must bring about a better understanding. Whenever a new international radio circuit or telephone line is ceremoniously opened to the public there are sure to be speeches by influential government officers who utter time-worn sentiments about international amity and who always "forge new links in the chain" that is supposed to bind countries together. Only a few months before the outbreak of war in 1914 there were some optimists who said that motion pictures would make international conflicts impossible. Films were to provide a sort of emotional outlet, a substitute for war which would satisfy any swashbuckler. No one has ventured to advocate that theory since, although the talking motion-picture has made the drama part of the daily life of tens of millions instead of the occasional influence that it was in the days of the Greeks or even in our own time when we were limited to the stage.

The truth is that there is not the slightest evidence of the creation of any better international feeling because steamship lines, railroads, airplanes, telegraphs, telephones and motion pictures spread ideas all over the world. Fast vehicles

and the newer means of electrical communication should theoretically make the whole world kin. In fact we have been told as much, although the wars that have been fought since the railroad and steamship were introduced about one hundred years ago, have been more frequent and terrible than those of any preceding century. Spreading culture proves to be one thing; growing to like an alien people is another. It is rather innocently assumed that the more we know about a people the better we will like them. Apply this principle to our own lives and see how it works. Do we like a corrupt political organization which battens on a city any better because we know it better?

It is difficult to see how the linking of the world into a brotherhood can ever be brought about by a mere interchange of ideas. Even in time of peace the press, the motion picture, radio are all utilized to confirm Frenchmen in their opinion that theirs is the greatest country in the world, to tell Americans that they are the cleverest and bravest people on earth, to convince Germans that they will rise after a crushing defeat to become even more powerful than of yore, and to let Russians know that it is their destiny to wipe out capitalism So long as human nature remains what it is, so long as nations deliberately apply the means of communication to foster chauvinism how can we dream of international peace? Consider the rather harmless utterances about the United States made by Sinclair Lewis and spread about by telegraph and newspaper. Whether they are true or false is not nearly so important as our own response to them. Small towns in the corn belt become indignant, and care little whether they are true or not. Apparently it is enough that they are what are called un-American.

If we examine the historic record critically it seems as if the better we come to know foreign nations the more likely are we to break the peace with them. The world is more crowded than it ever was both because there are more people in it and because distance means less than it ever did. But it does not follow that crowding and the shrinking of distances

must make us love each other more. The late James Bryce went so far as to say that had it not been for the extraordinary development of means of communication Europe would not have burst into a world-wide conflict with almost explosive violence. With the telegraph and telephone at work, with newspapers printing special editions almost hourly all European nations were simultaneously brought face to face with a crisis. In what are called "the good old days" when the stage-coach, the horse, and the sailboat were the ordinary means of transportation and communication, it would have taken months for a conflict to involve a whole continent.

At the outbreak of the last war the world had interchanged ideas for over five thousand years by means of writing, and for four hundred years by means of the printing press. The railroad and steamship had linked alien countries for somewhat less than a century. The telegraph had been in general use for at least fifty years and the telephone for about thirty. Motion pictures had been displayed on a world-wide scale for at least twenty years. When we make proper allowance for the tempo of modern life, thanks to engineering advances, we had the right to expect something better of humanity than what actually happened in 1914. All that the printing press and electrical means of communication had taught Germans, Frenchmen, Englishmen, Americans and Russians about one another counted for nothing. We saw strange alliances made, after much sordid bargaining, solely to secure selfish economic advantages.

The spreading of ideas implies tolerance. We must be able to listen to Russians explaining the beauty of communism without fury in our hearts, and Russians must permit us to explain how happy we are under a capitalistic system. But what do we actually find? The more potent the agency by which ideas are spread the more likely is it that a censorship will be established. European governments rule the press. Although censorship has been especially resented in England and America, books and newspapers are suppressed in both countries, when need be, on purely technical grounds, such as

violating the postal laws. Every motion-picture must be licensed. And latterly we have seen the introduction of something like a radio censorship. The point I am trying to make is this: As soon as we introduce a powerful means of mass appeal, national or international, we inevitably decree that it shall be utilized only to spread acceptable ideas. What is acceptable depends on the censor, public opinion, or the government. Under such a system there can be no real international understanding. Something is clearly wrong with the human mind.

After the potentialities of radioactive elements had been deduced early in the present century and a few physicists had gone so far as to calculate the amount of energy locked up in a thimbleful of matter Sir Oliver Lodge voiced a few doubts. Here, for example, was Sir F. W. Aston proving mathematically that there is enough energy in a glass of water to drive the Mauretania across the ocean and back. What if that energy could be released and controlled in the service of mankind? Sir Oliver Lodge was one of the few that refused to become excited by the possibilities. He doubted whether mankind was sufficiently enlightened to utilize so vast an amount of energy without destroying itself in some ghastly conflict. As we look back on the effect that the technical advance in the art of communication we cannot but be struck by the probable correctness of his low opinion of mankind. Our scientific and engineering progress is faster than our spiritual and moral progress. Technically we are demi-gods; ethically not much higher than the Goths who put Rome to the sword. We have assumed that because we have the means of understanding each other better we must of necessity have subjected ourselves to the process of understanding. What we actually need is a spiritual growth to match our technical growth. And that has no connection whatsoever with printing, photography and radio.

UNEMPLOYMENT AND ITS SOCIAL SIGNIFICANCE

By ARTHUR WOODS

(*Read April 23, 1931*)

ONE of the searching tests of a civilization is its success in making prosperous and happy the people who are its constituent members. It should provide for them conditions that are sound and stable, so that they can rest serene in the confidence that if they do their part all will be well. They will have free opportunity to vie on fair terms with their neighbors, under conditions which make it likely that the best man will win most, but that all who are competent, who will work well, who are honest and play their part, will find the chance to contribute their thought, their labor, to the common effort, and need not feel uncertainty or anxiety as to the future so long as they keep up their end. They need not fear the arbitrariness of a Sovereign, or of an oligarchy that holds itself in power by force or fraud.

Neither should they have to apprehend a harsh or unmanageable economic system that may unaccountably shift its needs or slow down its activities, so that they, the competent working members of the group, may be thrown in despair on their own resources, unable to find remunerative employment, unable to meet their contracted obligations, unable to provide food or clothing, or a roof for their own wives and children. They should not have to apprehend that through no conceivable fault of their own, in spite of their eagerness to work, their tried and proven experience, their competence and reliability as workers, they may become victims of blind, cold economic forces.

A free state, a free economic system owes its very existence to the fact that it provides and maintains conditions under which its members have the opportunity to earn their living in their own way insofar as that way is compatible with the

welfare of others, and of the group as a whole. It does not owe a man a living; it owes him the fair chance to make his own living.

If it were otherwise, if the state owed a living to each of its members, then the members, in return, would have to yield individuality of choice and occupation, and take whatever forms and conditions of living the state might prescribe. This is not our way. We believe otherwise, and have built and developed our system on the other basis of favorable opportunity for individual ambition and aspiration.

That is the American theory; it is the theory of most of the civilized world. But occasionally—too often—there come tides in our affairs that abruptly and insistently call that theory into question. We are in the deep waters of one of these tides now. Some millions of our fellow citizens are out of work. Try as they will, they cannot find work. They have plenty to offer. They are healthy, honest, skilled in some profession or trade, or in some detail of a trade. For years they have been successfully rendering service in return for salary or wages. Their success, and the buoyancy of business, has brought them better and better working conditions and larger emolument. Then, unaccountably, business has slowed its pace. Less production is needed; indeed wares already produced are not consumed. Industry after industry is more or less in distress; company after company has to tighten its lines, slow down its wheels, shrink its production, reduce its working forces. For a while employees may be on part time, then the evil day comes and they are no longer needed at all. Competent workers, eager to work, can find no work. There is plenty of capital, there are large—too large—plants and factories, full of silent machinery, there are plenty of workers of all kinds idle on the streets. The idle workers need the goods that are not being produced, capital needs employment, the factories are losing money daily and need to be manned. Yet capital stays idle, factories stay idle, workers stay idle.

Is it any wonder that people ask themselves why? Is it

strange that questionings arise as to what the reason is, why does this have to be, can nothing be done about it—*is* nothing being done about it? And if no satisfactory answers emerge, is it to be wondered at if the questions go deeper, and direct themselves at our economic order: is it what it should be; should it be changed?

Not many years ago industrial depressions were regarded as inevitable, as bound to occur from time to time, as veritable acts of God. There was nothing to do but batten down the hatches, furl the sails and ride out the storm. The captain of the ship was expected to have his vessel in ship-shape order, and to manœuvre it skilfully and courageously, but there was no criticism of him for failing to prevent the storm.

Now we are beginning to think otherwise. Perhaps depressions are not inevitable. Perhaps instead of being in the class of storms at sea these phenomena are preventable, more like plague, pestilence or famine, devastating in the toll they take, baffling in their complexity, puzzling in their manifestations, but in the realm of things that human beings can conquer. Yellow fever, unchecked and seemingly uncheckable, used to claim its thousands, and no one knew whence it came or how to avoid it, yet now, thanks to marvelous human skill and persistence, and noble human sacrifice, yellow fever is banished from most sections of the earth where it used to decimate its victims; and we think we see the days when there will not be a case of it on the face of the earth, when it will be a vanished, extinct plague. Are not industrial depressions in the same category?

Industrial leaders throughout the country are working on this theory. They feel that these recurrent catastrophes can be mitigated, can perhaps even be prevented, and they are studying the question in all its manifestations, and trying out different plans, making different experiments, with this aim.

The President's Emergency Committee this winter in Washington has felt itself in the centre of innumerable industrial laboratories, all working, each in its way, to diagnose and treat this fell industrial disease. This general

effort is earnest; it has already achieved notable results, and has pointed the way to further search. The extent and spontaneity of the movement, the assumption of the task by industry, the way in which employer and employed are working together, in the effort to find the causes, and the ways to combat this thing that plagues them both—these are all new manifestations. Never before has there been such an attack on unemployment; never before has there been such constructive progress made.

In one way the swing of the business cycle shows humanity following a natural course of behavior. People are apt to do what other people do, to follow where others lead. This human tendency has been so marked in the business cycle, the whole course of things is so much affected by what is commonly called psychology, that it has been suggested we should change the spelling of the word "cycle" to "psychle." This is what happens in the general business cycle: Times are good, people are employed, they have money, they spend it. To meet the demand, the supply rises, each manufacturer trying to sell all he can while the selling is good, trying to beat out his competitor, producing more and more so as to take every advantage of the consumers' demand and the rising prices. Everyone buys, sometimes paying, sometimes promising to pay. Not only do people have money, but they have the example of others for spending it. Spending is in the air; it is infectious—almost compelling—everyone is doing it: new clothes, a new radio, the latest model automobile.

Then on some grey day, someone wonders if he ought to pay so much for the piano; he postpones buying. Others feel the same way about other things they were thinking of buying, and the pendulum swings the other way.

It does not take much to make the change; one or two per cent falling off in sales makes the shop-keeper apprehensive, while deep depressions mean only about 15 per cent reduction.

With the pause in buying comes the choking of shelves

with goods that have been produced, or are still in process. Manufacturers grow uneasy, and slow up. Forces are reduced. There is neither so much money to spend, nor the desire to spend it. And the swing down begins; the depression is on us.

An era of industrial depression nowadays, while springing undoubtedly from many of the same causes as heretofore, is very different in its social effect from similar depressions of a generation or more ago. In those days a person out of work would be looked after by his friends and neighbors, his church, his clubs, his relatives. The children of a man out of work would visit various relatives or friends. The man and his wife would be taken care of likewise. The extra expense caused by having one more person in the household would be inconsiderable, and this friendly, neighborly method of meeting the situation would cushion the blow and would enable the man and his family to continue to live very much on the old scale, with no loss of self-respect, with no humiliation, with no feeling that others were doing for them anything more than they would do for others if the tables were turned. It was a natural, wholesome, individual way of meeting a great social need. There was no question of public relief; the man had lost his job for the time being, but he had lost nothing in social standing, nothing in self-respect, nothing in standards of living. In this way the country was able to progress from generation to generation, to emerge from one industrial depression after another, with great loss, yes—with deprivation, hard economies, many anxious days and nights,—but without slowing up the march of progress that has characterized our country; the upward march toward higher standards of living, better working conditions, shorter working hours. Indeed, there have been only temporary halts in the trend toward providing better and better conditions under which all classes of people can carry on their work and their play, raise and educate their families, make the most of themselves and contribute to the welfare of others.

As we have grown away from a rural civilization into

great urban communities, and people have become less individuals and more units in a great system, as we grow toward mass education, mass production, chain business—so the method of life changes. In the depression which we are now working out of, for illustration, more and more people out of work felt that they ought to obtain the relief they needed from private charities or public relief measures. The old practice of having families and friends absorb the need has in many ways seemed to be yielding to the newer practice of looking to public funds for relief. Indeed, one is inclined to believe that the apparently larger numbers of people in distress this year may seem larger not because of an actual count of heads, but because more and more people in proportion to older times are looking to public means for relief.

From the point of view of the man out of a job the newer method is far inferior to the old. Nowadays he casts himself on charity, on public relief, and the only kind of help he gets is in kind or in money. The sympathetic helpfulness of family and friends is not replaced by the impersonal supplies of charity or of governmental agencies.

We have been so much impressed by the widespread and severe personal suffering in such a business depression, with its lasting consequences in impaired health and morale, and with the increasing difficulties of giving adequate help of a kind that constitutes real relief, that we look more and more for means of preventing the oncoming downward swing of the cycle.

While the depression is on, of course, everything must be devoted to relieving the distress it causes, and nothing could have been more heartening this winter than the way in which the country as a whole has risen in answer to that call. States have organized state committees, cities have done likewise; towns and counties, wherever there has been need have developed special organizations to meet it. This has all been done spontaneously, each community recognizing its obligation toward its own members, and those who were better off giving of their time and their money to help those

who had lost their jobs. The work of these committees has been vigorous, intelligent and far-reaching. Each has worked in its own way, for in each community the need was somewhat different, but the determination in each community was the same: to meet the local need no matter what it might be, or what it might develop into. The country will never know what it owes to the individual men and women, who as members of these committees, or working with the committees, have given the best that was in them to try to meet the need as it showed up in their localities.

This sort of thing is a very hard fight, for the need grows larger the more the situation prolongs itself; the longer industrial life remains in the doldrums, the greater the needs become of those who have lost their jobs and those who continue to lose them, while on the other hand the prolonged and growing strain on funds tends to make it harder and harder to raise money for relief.

We find running through all the work of the committees and groups that have been working during the depression the insistent question—"What can be done to prevent these catastrophes, or at any rate toward warding them off longer, and lessening their violence if they must occur?" This is rather a new feature of an industrial depression in this country, and it is one of the live public questions today. The efforts that are being made in industry which I have just described have been directed toward trying to answer this question, as well as toward trying to meet the needs of the present situation, and the new trend in industry in both these directions has been very clear.

To mitigate the immediate evils caused by the slowing up of industry, there has been a marked movement in companies away from the older customs. Not many years ago, when business began to slow up a company would discharge all the men it could,—without any thought of their welfare,—would lower the wages of those it kept so far as it dared, would postpone all construction or reconstruction and all repair or maintenance work that it could. Only then would it feel

that it could go to its bank with the confidence of having credit lengthened.

This year we have noticed a very marked change in the action of companies. In some instances it has gone only a very short way, but as a trend it is marked and growing. Instead of discharging everyone possible the tendency is to see how many can be retained. If men have to be discharged, the effort is made to pick and choose among them, selecting for discharge those that will be hurt the least by it: unmarried men, for instance, men who could qualify for a pension, men who would be most likely to find jobs elsewhere. And in their own several ways the companies continue to take an interest in the welfare of discharged employees: by donating supplies, by allowing them to stay in company houses, by making allowances of fuel, by supporting their credit at the local shops, by making individual loans without interest and to be repaid only when the man gets his job back. Instead of putting off all construction and repair work, the tendency has been to anticipate such work, so as to provide occupation for people instead of having to discharge them. And this tendency in industry has come about not because our industrial organizations have suddenly transformed themselves into charitable associations, but because it has been deemed good business.

Striking as is this trend of industry to grapple with the existing emergency, the efforts that are being made to study the question with reference to the whole cycle are no less notable. And it is the characteristically American method of working out a difficult problem: to have people working on it each in his own way all over the country, so that gradually the successful methods will emerge, and all can take advantage of their benefits.

One great tendency in industry in its effort to work out preventives, is the study of unemployment insurance. The question is being raised as to whether industry should not protect itself against the evils and difficulties of unemployment just as much as it protects itself against fire and theft. Be-

sides trying to arrange its plant so as to reduce the dangers of conflagration and thieving, it insures itself against them. Besides trying to stabilize and regularize and rationalize its business so as to make shallower the trough of industrial depression, why should it not insure itself against the catastrophe which follows in case these measures are not sufficient? Not very great actual progress has been made along this line, though the matter is in such a condition that at any time the present efforts may result in bringing to light and making available for putting into practice, well considered and effective methods of industrial insurance. One of the most promising signs that have come to our attention recently has been that a number of companies have adopted identical plans according to which the company lays aside funds every year, and undertakes to pay employees who are laid off 60 per cent of their wages for a certain number of weeks, the time being dependent upon length of service. The companies announce the intention, if the unemployment situation should go on and be too much of a strain on the fund, of increasing their own contributions and calling for a contribution of similar amount from those who still hold their jobs.

Another subject that is being studied with a view to its usefulness in lessening the violence of industrial depression is business statistics. We now know many facts about industry that were not known or were only imperfectly known in previous times, and these facts are published periodically. We know about the number of freight cars that are loaded each week, the amount of loans that are made by brokers to their customers, the deposits in savings banks, the earnings of companies, and we are learning more and more about the significance of such figures. It is not too much to expect, however, that the profession of assembling and making available the significant facts about trade and industry will develop very fast, and that information which we are at the present time without, may be available shortly and may be most helpful in making it possible for people to see ahead the up and down trend of business, so that measures may be taken accordingly.

We do not know today, for instance, how much money is due to dealers in the way of consumers' indebtedness. This includes installment purchases, loans to finance consumer purchases, borrowings on insurance policies, loans from pawnbrokers,—in short, indebtedness that is incurred to purchase articles that are to be bought and consumed,—as contrasted with producers' loans, which are for commodities that are to be manufactured into goods for subsequent sale. The best figure I have been able to obtain is ten and a half billions of dollars, a truly huge and almost incredible sum. If any such sum of money is due in this way it means that it will have to be paid, new money paying for old consumption, before there can be much increase in consuming power. With reference to stocks of goods on hand, again we have certain information, but we have no idea what stocks are in private hands and therefore cannot take them into consideration in our estimate.

Again, it may not be too much to hope that some forms of information with reference to industry may be devised by thoughtful and resourceful statisticians which will show, in ways which we now do not imagine, the course of affairs in business so as clearly and unmistakably to point out ahead of time the tendencies toward change.

Much consideration is being given to the laws under which American business operates. In the main these laws are old. They may have met the needs of industrial rules and regulations years ago, but they may nevertheless be misfits now and may be working against the interest not only of industry but of the public. One of the great causes of the over-stocking of shelves and overproduction probably is the instinctive effort of manufacturers in times when business is good, while people are buying, to get their full share of the business and a lot more. They enlarge their plants, they increase their production to get ahead of their competitors. Their competitors are doing the same thing, and when the evil day of slowing up comes on, all of them are caught with large supplies of goods which they cannot sell at a profit.

People are wondering whether such ruinous competition is not one of the causes of industrial depression, and whether the time has not come for a thoroughgoing examination of the laws by which corporations are regulated, to the end of removing the necessity for ruthless and ruinous competition, yet at the same time protecting the rights of the public. Under the present situation everyone suffers, employer and employee both. The whole system is plainly upon trial to see whether it has in it the capacity to operate industry as it exists now, for the benefit of all concerned—the owners of companies, and their name is legion, the managers and the workers.

Experiments are being made in public employment offices to see what is the best way to provide the means by which the job, if there is a job, finds the man who is fitted to fill it. In this city of Philadelphia very promising work is being carried on along this line, and also in Rochester, New York, Minneapolis and St. Paul, and in many other places. When these local efforts are all knit together into a successfully handled, smoothly-working national organization, we shall at any rate come nearer knowing the facts of unemployment, for people will register at such bureaus.

Closely allied with the work which would be done by a national system of employment agencies is the benefit that could be gained from work now going on in vocational guidance and vocational training and re-training. With the development of industry toward specialization and the narrowing of the kind of job that an individual man can do, it becomes harder for him to find anything that he can turn his hand to except the narrowly specialized job which he has been trained to perform. Much of this difficulty can be mitigated by guidance and by training. Particularly those beginning work can be guided to start work where there is most prospect of successful and permanent employment. The supply which would otherwise fit itself for overstocked occupations could thus be thinned, and young people might be turned into occupations promising greater opportunity. Workers could

also be trained so that they might be able to turn their hands to several different kinds of jobs instead of only one.

Such guidance work and such training is being carried on now by some state vocational systems and by some industrial companies. It does not yet seem clear which is the better arrangement or whether each has its proper field, but earnest studies of the question are now being made, and the results cannot fail to be helpful.

It did not use to be necessary for us to look beyond the boundaries of our own country for anything that would affect us in the way of industrial booms or depressions. Now, however, we find, for better or for worse, that what affects other countries affects us also. We are closely connected with them by the forms of modern life and by the modern inventions. These bring us so near in every way that we cannot escape from the effects of world inter-dependence. We must have dealings with citizens of foreign countries whether we choose to or not. We find that what affects them favorably affects us favorably, and vice versa; for nowadays the affairs of different nations are like various streams of water seeking relentlessly a common level.

Is it not inevitable that the transition in political thought and administration from the national to the international outlook will be carried over into industrial affairs also? We can at any rate be certain that the course of our industries is markedly affected by conditions in the rest of the world. Since the home market doesn't seem to be enough, the only alternative is trade relations with the rest of the world. These, to be successful, must be arranged on the basis of mutual benefit. If industry can work out this problem it will have contributed decisively, not only to the industrial good health and well being of this nation, but to the greater cause of peace and good will on earth.

THOU SHALT NOT

By JAMES M. BECK

(*Read April 23, 1931*)

The title of this address is not of my selection and leaves me in some doubt as to the exact subject which the Society desires me to discuss. I shall assume, however, that it is the justice and efficacy of prohibitory laws.

As this is the birthday of Shakespeare, who is traditionally supposed to have violated the poaching laws of his own time and thus made possible his subsequent career as a dramatist in London, I may be pardoned for two quotations from his masterful mind. In one he describes the prohibitory laws of his own day as follows:

> "Laws for all faults,
> But faults so countenanced that the strong statutes
> Stand like the forfeits in a barber's shop
> As much in mock as mark."

And the other his admonition,

> "We must not make a scarecrow of the law
> Setting it up to fear the birds of prey—
> And let it keep one shape, till custom make it
> Their perch, and not their terror."

The first quotation felicitously described the present condition of our statute books; the second serves to give to this generation the much-needed admonition that a penal code, which unduly attempts to regulate the life of the individual, simply serves to "make a scarecrow of the law."

In limine, let me first challenge two mistaken beliefs, which are of such general acceptance that not only the Babbitts, but even wise men regard them as indisputable.

The first is the common statement that Congress and the legislatures of the Federal Union are daily grinding out thousands of laws to regulate the conduct of individuals. I have

often heard it stated by eminent jurists, who ought to have known better, that the annual grist from our legislative mills exceeds 400,000 laws. This unchallenged statement has always made a deep impression upon European observers, who are too apt to regard our governments, State and Federal, as a Fool's Paradise, in which thousands of 'prentice hands are exhausting their energies in formulating new regulations of private conduct.

I believe, on the contrary, that our legislatures contribute very little each year to substantive law. Many thousands of statutes are unquestionably enacted, but nearly all of them are purely administrative in character. A law, in the sense that I shall treat it, is a rule of conduct, which, to use Blackstone's phrase, "commands that which is right and prohibits that which is wrong." When Congress, for example, passes a Rivers and Harbors Act, with thousands of items and many provisions as to administrative details, it does not add anything to substantive law, but simply makes purely administrative provisions for the continued efficiency of the governmental machine. At the end of each Congress I am generally surprised to find how little that is novel has been contributed to substantive law, and, while our state legislatures occasionally pass "freak" legislation, which the comic press satirizes, yet the fact is that statutes, insofar as they seek to provide for the reasoned adjustment of human relations by commands and prohibitions, have a very small annual increment.

Undoubtedly, we have on our statute books, as an accumulation of many decades, an immense volume of experimental legislation. The immortal founder of this Society, in his scheme of government for Pennsylvania, wanted a decennial revision of all laws, even of the Constitution, to eliminate all demonstrated follies. Franklin's idea was suggestive, even though now impracticable in a busy age, but the American people subconsciously take the position that laws that are obsolete and outworn will not be enforced anyway and therefore it is useless to destroy their purely nominal authority. Thus, there are floating in the sea of government many

legislative derelicts, which are a real menace to navigation, for one never knows whether a "blue law," that was enacted in a different century and has long since been ignored by common consent, may not suddenly become a weapon of oppression in the hands of a too-zealous and ambitious public prosecutor. The "blue laws" of Pennsylvania remain substantially as they were in the days of Franklin, but are generally regarded as a dead letter and yet such a law can be revived at any time—because technically unrepealed—when some special interest or some fanatical reformers desire to invoke its full rigor. In such event, the Judge cannot recognize a repeal by tacit consent, but can only solemnly say: "*Sic ita lex.*"

The second error is the equally common and unchallenged statement that we are the most lawless people in the world. As a people, we are too patiently submissive to unwise, outworn and unjust legislation. It is true that in certain instances, notably Sabbatarian and sumptuary laws, the people have enough of their ancient instinct of liberty to ignore laws, which they believe unjust, and they do this even though the law has had the highest sanction of the Constitution itself. The Eighteenth Amendment—as was the Fugitive Slave provision of the Constitution—is an affront to millions of self-respecting Americans and now, as then, a considerable minority refuse obedience. While this is theoretically indefensible, yet, as Edmund Burke once said, "You must pardon something to the spirit of liberty."

Eliminating, however, these extreme cases, where the laws have caused a deep-seated grievance in sections or among numerous classes, and further eliminating the incorrigible and almost congenital habit of returning tourists—especially women—to avoid the payment of custom dues, if possible, the fact remains that, as to the great mass of statutory regulations, the American people accept them as the rules of the game and obey them, if they know of them. Even in the vexed matter of prohibition, it is an amazing fact that so many millions of people have accepted the Eighteenth

Amendment and conformed their lives to its requirements, and this without acrimony or too vigorous dissent. If an attempt were made to enforce such a law as the Volstead Law in France, Germany or Italy, it would probably result in revolution. Even in England, where the respect for law has been strengthened by centuries of habitual obedience, a law similar to the Eighteenth Amendment would overturn any government of the day. The American people, because they labor under the illusion that they themselves make the laws—which, in fact, are made by their representatives—and because of their belief in and affection for their Government, generally obey a law, if they know of its existence, with cheerful resignation. This may be partly due to the belief that they can change the laws, or by common consent can regard them as obsolete. There is no better evidence of the general law-abiding character of the American people than their patience and restraint during this terrible period of depression.

I recognize that the most portentous sign of our times is the highly developed organization of the under-world in all of our cities, which has resulted in a wave of crime that is beyond any precedent. But, while many may suffer, the thugs and bandits are few in number, as compared with the great body of the population. Their evil power is due to the fact that crime is no longer an isolated phenomenon, but is now a highly organized industry, even as thuggery, from which we get the very name of "thug," once was in India. That crime should have become a major industry on the part of, say, 25,000 professional criminals, is due in large measure to the demoralizing influence of national prohibition, which, unquestionably, has lessened, but not destroyed, the law-abiding characteristics of the American people.

In the discussion of this subject, it has often been charged that the crime wave is due to the defective procedure of our criminal courts and even the Bench and Bar have been charged with the greatest responsibility. Undoubtedly, our code of criminal procedure needs revision and both Bench and Bar are not wholly free from fault. Undeniably, justice is not as

swift, summary or certain in our criminal courts as in England. This is partly due to the pernicious results of an elective judiciary and partly to a perverted idea of the true function of the Bar in the trial of criminal causes, but the responsibility of both Bench and Bar, in my judgment, is a minor factor in the problem.

A more contributory cause is the mistaken humanitarianism, which has turned our prisons into desirable hotels and social clubs. Punishment does, to some extent, lessen crime and for the professional criminal it should be punishment, and not a temporary vacation under agreeable surroundings. It would be a good thing if the professional thugs and assassins, who are daily robbing banks and resorting to murder when necessary, on a second conviction should be sent for life to an outlying island. Society must take into account the portentous fact that the automobile and the automatic pistol have made the prevention and punishment of crime exceedingly difficult and the punishment should be adequate to the crime. It cannot be so, in respect to such professional criminals, by probation laws and luxurious prisons.

The causes of lawlessness lie much deeper and find their ultimate source in the evils of a complex and mechanical civilization, whose gross materialism has so greatly impaired the spiritual faculties of man. The old conceptions of right and wrong have been weakened by the artificial institutions of a mechanical age. Take, for example, that beneficent creation of the law, which we call the corporation. From an economic standpoint, it has been of immeasurable benefit to human society in enabling men to combine their resources to achieve some end that was beyond individual enterprise, but few appreciate how destructive it has been to the spirit of morality in divesting the individual of any sense of moral responsibility. As officers, or even stockholders, of a corporation, men will condone acts, or accept the evil fruits thereof, which they would disdain to do if it was a transaction between individuals. In too many corporations, the sense of moral responsibility is diffused to the vanishing point, for the

wrongs it does are chargeable to the community of interests and no one man feels responsible. In a sense that Coke never meant it, many corporations have no souls.

All this does not disprove my contention that the great mass of the American people generally accept the obligations of law with a cheerful obedience that is exceeded by few nations. Nevertheless, we remain individualists, who claim the right of private judgment as to the wisdom of laws and we therefore cannot, in the intellectual sense, respect a law unless it be worthy of respect, in other words, reasonable. The primal requisite of "reasonableness" is that all laws should be fairly within the true province of government. This is not determined, finally or conclusively, even by Constitutions. There is a higher law, under which an individual can say that the power of the State to determine his way of life is beyond the fair province of government.

The Pilgrims of the "Mayflower" had this conception, for, when they signed the famous Compact in its cabin, they did not agree to obey *any* laws that the majority would enact, but only "just and equal" laws. Deep down in the instinct of every reasoning man there is the feeling that the State has no unlimited power to impose its will upon the individual and that, when it attempts to do so, it has exceeded the limit of its moral authority, even though it have the legalistic sanction of being the presumed will of the majority.

This is the very essence of liberty and all the great struggles for human freedom have involved a challenge to the moral authority of the State to impose its will upon the individual in matters that are beyond the true province of government. When it does so, we call it "tyranny" or "oppression," and the advance of civilization can be traced by the extent to which individuals have said to the State, whatever its form or whatever its pretended sanction, "There is a limit to your authority; hitherto shalt thou come, but no further; and here shall thy proud waves be stayed."

The difficulty is to trace the boundary line, which divides the just discretion of the State to protect the common welfare

and the right of the individual to pursue his own life, and, in trying to draw this fateful line, no one, so far as I know, has gotten much beyond glittering generalities. Jefferson inflamed the world by his noble Declaration, whose mighty reverberations are still heard, that each man is "endowed by his Creator with certain unalienable rights, among which are Life, Liberty and the Pursuit of Happiness."

Eloquent as this was as an appeal to the conscience of mankind, and profound as its effect has been, for good and for evil, upon the world, it does not advance us very far in drawing the line between a reasonable and an unreasonable authority of the State. If we turn to John Stuart Mill's classic work on "Liberty," we find a far more closely reasoned discussion of the question, but, nevertheless, the great English philosopher soon finds himself attempting to solve the question by lofty generalities, which help little in the discussion, except in so far as the application of his generalities to specific laws indicates where he would draw the line in particular cases. His ultimate truth is

"that the sole end for which mankind are warranted, individually or collectively, in interfering with the liberty of action of any of their number, is self-protection. That the only purpose for which power can be rightfully exercised over any member of a civilized community, against his will, is to prevent harm to others. His own good, either physical or moral, is not a sufficient warrant. He cannot rightfully be compelled to do or forbear because it will be better for him to do so, because it will make him happier, because, in the opinions of others, to do so would be wise, or even right. These are good reasons for remonstrating with him, or reasoning with him, or persuading him, or entreating him, but not for compelling him, or visiting him with any evil in case he do otherwise. To justify that, the conduct from which it is desired to deter him, must be calculated to produce evil to some one else. The only part of the conduct of any one, for which he is amenable to society, is that which concerns others. In the part which merely concerns himself, his independence is, of right, absolute. Over himself, over his own body and mind. the individual is sovereign."

Possibly this is as far as any rule can be stated, except when applied to a given concrete illustration, but if the self-

protection of society, meaning its well-being, is the test of its powers, then there is no activity of man, from his cradle to his grave, that society could not regulate, on the theory that the welfare of the unit of society is the highest concern of the State. Thus the State, assuming it to be the ultimate judge of its own welfare, can govern the life of the individual from considerations of eugenics to that of the mortician.

The fact is that this question of the true limits of government is largely one of degree and does not admit of definition, even when so great a man as John Stuart Mill attempts to define it. All jurists know that there are many fields of the law where the capacity to define anything ceases and all that the jurist can say is that a given act must be "reasonable." Possibly the greatest volume of litigation turns upon this simple question and while, theoretically, it depends upon as abstruse considerations of relativity in the moral world, as Mr. Einstein's investigations in the physical, yet in its practical application, it depends very largely upon enlightened common sense. Each man, in proportion to the development of mind and soul, has a greater or less instinct as to what is "reasonable."

For example, the moral justification of sumptuary laws cannot be determined by the generalization that the State has no power to say what a man shall eat or drink. The State may well have the deepest concern in what he eats or drinks. If he is a drug addict and takes cocaine, heroin, absinthe, or other soul-destroying drugs, the State is not only justified in ruthlessly destroying the traffic, but, as example is contagious, it can properly restrain a man, or even punish him, for taking a drug which, in destroying him, is calculated to destroy others.

While it does not follow that there is any moral justification for prohibiting the use by the individual of alcoholic beverages, yet the two cases differ only in degree. In the former case, no one can safely take these pernicious drugs; there are no advantages to be balanced with the evils. But when we consider alcoholic beverages, which men for thou-

sands of years have taken, the fact is indisputable that millions have enjoyed a glass of wine or beer with no appreciable disadvantage, either to themselves or others, and possibly with some advantage, and the problem of the law then is whether all, who innocently and harmlessly use the fruit of the vine, should be prevented from doing so because others—few in comparison—abuse it. Here no political definition of liberty can solve the problem; it is purely a question of degree and must be determined by an enlightened common sense. The State must weigh against the abuses of alcoholic beverages the harmless, if not beneficial, uses and, of course, the State, apart from the question of its moral authority, should also consider whether such a law may not do more harm than it does good in provoking the very appetite which it seeks to destroy, for the State must take into account the instinctive revolt of the individualist against any law, which he regards as in excess of the true powers of government, especially when rooted in the racial habits of centuries. I of course recognize both the right and duty of the State to regulate drastically the commercial traffic in intoxicating beverages.

This Society, founded by Franklin, could, I think, profitably consider in future discussions this nice question, how to reconcile the just claims of authority with the liberty of the individual. It is the old question that was answered by the Master, when he said: "Render unto Caesar the things that are Caesar's and to God the things that are God's." Difficult as the problem is to determine the line between authority and liberty in the absence of practical application to some concrete illustration, yet, inasmuch as the average man is apt to consider laws too much from the standpoint of pragmatic advantages and too little from the great fundamental principles of liberty, it is important for those, who think more deeply, to consider the fundamentals of the problem and thus to arouse in the minds of the people, on the one hand, a respect for just authority and, on the other hand a jealous insistence upon individual liberty.

Undoubtedly, as society grows more complex and the public conscience becomes increasingly sensitive, there is

need for ever greater and greater invasions of individual liberty. In democratic governments these invasions upon the rights of the individual are, theoretically, determined by the people. They should not determine these questions purely from the standpoint of pragmatism. They should be taught not merely to respect the just authority of the State, but to guard jealously the sacred rights of the individual. Never was there greater need of such an enlightened public conscience than now, for the spirit of individual liberty is today overshadowed by the authority of the State and the average man complacently accepts many unreasonable laws on the theory that it is the will of the people, but as Edmund Burke showed long ago, the tyranny of the majority is just as indefensible as the tyranny of a despot.

It would contribute much to a rational view if the average man had a clearer conception that this reasoned adjustment of human relations, which we call "law," is not solely the province or within the power of the political State. As Caesar divided Gaul into three parts, the rational man could recognize that law is itself divisible, in its sanction, into three parts. These are the Church, Society in its broadest sense, and the political State, and, measured by efficacy, the political State, in attempting to impose mass morality upon the individual, is often less efficient than the other moral agencies. The Church, with its sanction rooted in the belief in the supernatural, is of great, although unmeasured, potency in regulating the morality of the individual, but the power that is far more effective than either the Church or the political State is that Social State, of which Proudhon said, as early as 1845, that it was a "living being, endowed with an intelligence and activity of its own and as such an organic unit."

Few are disposed to question either the sanction or the efficacy of those social laws, which we call "customs," and to which the German philosophers have given the word "Sittlichkeit." Our daily habits conform to the imperious demands of Mrs. Grundy far more than to the mandates of the political State. That tyrannous despot tells us what to

wear, what to eat and how to behave and often reduces us to the level of little children in ordering our lives.

When the "Titanic" went down, all its passengers became immediately and instinctively conscious of a social regulation that, in saving lives, women and children should have preference. Who made this law? It was not the result of any contractual agreement, for probably few of the ill-fated victims of that great disaster ever considered the question until the terrible exigency suddenly confronted them. It did not arise from utilitarian considerations, for presumably, the lives of the male passengers were pragmatically of equal value to society. The law was something more than a sense of individual morality. It had compelling power. All were conscious of its obligation and all obeyed it, although obedience meant death to many. In that wild struggle for life, few would have given any heed to a statute of the political State. As the waves rose upon the doomed vessel, they would have said, as the boatswain in "The Tempest," "What care these roarers for the name of King?" Nevertheless, what the political State could not do in imposing its will, the social law of "Women and children first," was of compelling authority. Men died in obedience to it.

It would be well if the proponents of State-made laws, which seek to impose mass morality upon the individual, would recognize that their true medium of reform was Society and that this great authority is only impaired when the political State tries to supplement it. For example, time was in this country when a young man who drank was viewed with disfavor and a young woman became *declassé*. Before the era of prohibition, the younger generation was thus measurably protected against the evils of intemperance by social mandates. When the Eighteenth Amendment usurped the province of the Social State, the authority of the latter was weakened. Temperance can be better promoted, if Society makes drunkenness an unpardonable offense than by police measures of the State. Mrs. Grundy can be more potent than a policeman's club.

The reasoning man and woman must therefore recognize

that to the political State belongs its very limited functions; to the Social State the larger, but still its peculiar functions, and to the Church its functions in enforcing the moral code by the highest possible sanction of supernatural command. To fail to heed the Master's injunction to "render unto Caesar (the political State) the things that are Caesar's" and to religion and society the things that are peculiarly within their province, is to bring about chaos and provoke irremediable confusion.

Speaking jocosely, many might welcome laws that would punish the social nuisances, which the immortal Koko, in "The Mikado," had upon his list and if the political State is to reform the world by remoulding the moral nature of man nearer to its desire, then I would like to see all the radio crooners smothered in boiling oil and an especial guillotine ready at all times for those who abuse the channels of the air with noisome advertisements. But, speaking seriously, the attempt of the State to reform the moral nature of man in all his social peculiarities is always ineffectual and, indeed, is pernicious, because it lowers the moral power of the individual to work out his own salvation. Shakespeare summed up in one immortal phrase the folly of those, who, not content with conforming themselves to their conceptions of morality, try by State-made laws to compel conformity on the part of others, for Sir Toby, even though in his cups, well said to the puritanical Malvolio, "Dost thou think, because thou art virtuous, there shall be no more cakes and ale?"

Those who framed the Constitution understood this necessary division of authority, for it not only said "Thou shalt not" to individuals, but to the State itself, and it sought to conserve and develop the spirit of individualism by constitutional limitations. But, alas, that spirit is largely spent. While the men who framed the Constitution thought generally in terms of abstract rights, we today think in terms of practical economics and therefore the individual only weighs the pragmatic advantages of a proposed law, without any due consideration of the sacrifice of a fundamental principle, which involves his liberty. In this respect, as in all respects, truly "Eternal vigilance is the price of liberty."

ROUND TABLE DISCUSSION

DOCTOR CONKLIN IN THE CHAIR

THE first question was put by Arthur H. Compton, University of Chicago who asked, apropos of Dr. Lee K. Frankel's paper of that morning about the life span, "whether there is any known tendency for the period of youth in the human race to be extended."

To this Doctor Donaldson replied that in one mammal (the rat) which has been carefully studied, the same fraction of the span of life as in man was given to the maturing of the brain and the completion of body growth, and that there was nothing to indicate that these fundamental relations were open to any material change.

REMARKS OF DR. CYRUS ADLER AT THE OPENING OF THE ROUND TABLE CONFERENCE

The Secretary of the Society has requested me to say a word at this Conference which is intended to discuss as a whole the papers presented during the meeting to-day under the general subject "This Changing World." It is of course impossible in any time that one speaker can have at his disposal to say anything that would cover in a comprehensive way the very remarkable and interesting group of papers presented at these two sessions. I shall therefore single out but one or two points.

During the morning session, one of our biological friends presented a paper which in effect portrayed a purely mechanistic theory of life, and in the course of the paper it was indicated that from his point of view the only subjects worthy of serious consideration, if I understood aright, were those strictly within the scientific field which consists in the gathering of facts and the drawing of deductions therefrom in order to form a working hypothesis for "This Changing World."

In the course of his remarks, he even indicated that the older studies like theology, archaeology and even history were from the scientific point of view in a sense parasitic, but he later modified the mechanistic attitude with the statement that such things as art and music had spiritual values and must be cultivated for the good of our souls.

Devoted as I have been to these so-called parasitic branches of knowledge, which in more polite phrase were called the humanities, I naturally feel the proposal of the mechanistic theory trenches somewhat upon the field of the older studies and employs their phraseology without accepting the connotations of that phraseology. Thus it would seem to me that a person who believes that all the processes of thought, all the sensations, the things that we call intellect, memory, etc., are purely chemical reactions, is misleading others if he employs words like "soul" and "spirit" which have their definitions, even though it be difficult to give the definitions in terms which would be acceptable to purely natural scientists.

It might be a fair thing to ask whether the great accumulation of facts which has been going on during the many centuries has really improved the actual intellectual ability of man. I think that it would be hard to establish that these accumulations of knowledge have produced a greater intellect than Aristotle or that the improvement in taste has resulted in a greater sculptor than Phidias.

The humanities which are in the truest sense of the word liberal,—we used to call them in our College discipline the liberal arts,—always welcomed and took into account the discoveries of scientific men and indeed they have sometimes touched these discoveries with a sense of poetry. The philosophers and theologians of the Middle Ages saw God, or if you choose the great soul of the universe, through the celestial phenomena that were known to their unaided eye and it would seem that this grand vision of the medieval scholastics was being repeated in our own day.

If I had to choose for a vision of the universe as between biologists and astronomers, it would seem to me that the man

who looks through a microscope is apt to have his vision focused, maybe of necessity, upon the very smallest of objects, whereas it is from the astronomer, the man who looks through the telescope, that we are now really getting grand and inspiring visions of the universe.

The older knowledge, as I have said, was liberal. It consisted in the study of language, literature, philosophy and history, but it always had an open mind to the great facts of nature and welcomed them and gave full faith to them as a contribution to the mind of man and to human thought.

If the mechanistic theory were carried to the extreme and there were produced, as I understand there can be produced in the laboratory, a robot that could in every way duplicate the acts of what we call man, it has been suggested, and I regret that I cannot take credit for this suggestion, that the acid test as to the identicalness of the real man and the mechanistic man is whether the latter would ever engage in the search after truth, which, in the last analysis, is the purpose of all investigators.

Remarks Made at the Round Table Discussion by Robert A. Millikan

I had not been asked to take part in any way in this discussion, and should not do so if the Chairman, Professor Conklin, had not requested it. But the discussion is already bringing to light a fact often times overlooked by those who call themselves "materialists" or "mechanists," namely, that the discoveries in physics of the last few decades have taught the physicist that a larger degree of vagueness or indefiniteness inheres in his own attempts at sharp definitions than he formerly supposed.

The terms "material" and "spiritual" are often contrasted as though the former were perfectly definite and sharply definable and the latter merely a qualitative concept, but the modern physicist has been obliged to give up that sort of distinction. We have been talking here glibly about atoms, for example, but at the present time I have no hesitation in

asserting that no precise and sharp definition of an atom can be given. We formerly defined it as one of the seventy-odd ultimate units or elements out of which the earth was supposed to be built, but the discovery of the facts of radioactivity rendered quite meaningless that definition, and we now generally replace it by the statement that "the atom is the unit of chemical combination." That is a perfectly satisfactory dictionary definition; but if we are asked, then, to explain what we mean by chemical combination as distinct from physical attachment under the influence of cohesive forces, we find that by using the new definition we have done nothing except to push off the vagueness or indefiniteness from the word "atom" to the word "chemistry," for nobody today can possibly make a sharp line of differentiation between physics and chemistry despite the fact that *broadly* speaking these two fields are capable of rough distinction.

Again, we used to define matter by the property of inertia, but a few decades ago we found experimentally that ether waves also possess inertia, and the sharp distinction which we had been making between matter-physics and ether-physics simply disappeared. Today the newly observed properties of matter seem to require us to describe it in terms of waves, although for ordinary purposes the old inertia-definition is sufficiently satisfactory. Also, the so-called Einstein equation, which seems to have experimental credentials, has forced us to abandon the attempt to maintain any fundamental distinction between "matter" and "energy." Today I do not know, then, how any physicist can give you a sharp definition of the word "matter." In other words, material and spiritual are no longer distinguishable on the basis that one is a sharp, definable, quantitative concept, and the other a mere quality.

The net result of the developments of modern physics is to teach physics a lesson of modesty, such as it did not formerly possess. We are now recognizing the limitations of our knowledge, and are losing that dogmatic quality which nineteenth-century physics so markedly possessed, a quality which

I suspect we inherit from medieval theology and which when we had outgrown it we passed on to biology, which, so far as I can see, seems to be dominated with that dogmatic spirit more now than are any of the physical sciences, perhaps more than theology itself. Physics, among all the sciences, seems to be now teaching science the limitations of its method and calling out to all scholars in all fields that it is well to remember that we are still finite human beings, who have gone a certain distance, it is true, in transforming ignorance into knowledge, but who have not yet made it possible sharply to define either the words "matter" or "spirit," although both words are sufficiently understandable for most of the purposes for which we use them.

Remarks made at the Round Table Discussion by W. F. G. Swann

The question of the significance to be attached to the reality of atomic models is one which must be handled with considerable care. The particular properties which it is the object of a model to provide are often not uniquely characteristic of the model itself but of a much larger range of properties. The danger inherent in the assumption of an exact model lies in the fact that the features of the model which are really irrelevant, may demand, on their own account, something which would be contrary to the properties of the atom itself. I have sometimes meditated upon what I have termed the "irrelevance of the obvious." Suppose that I set to a high school boy a problem concerning a ball which is hurled into the air and the question at issue is how long it will take the ball to reach its highest point when thrown upwards with an assigned initial velocity. I can well understand someone whom I had asked to work out the foregoing problem about the ball, asking me in turn what the weight of the ball is. I tell him the weight doesn't matter, but he doesn't like it because some of the reality of the ball has vanished from his vision with the weight. He asks me for the color, and I tell him that doesn't matter, and I add to his troubles by telling him that I

will withdraw even my remark that it was a ball, and leave its shape indefinite. Then, if he is overmaterialistically minded he will explode entirely and demand to know how he is to work out any problem about the body if I won't tell him its color, shape, or mass. There is nothing left for him to think about, and he may well claim that it is difficult for the human mind to think at all unless it has something to think about. Well, to please him, I tell him the body is red, weighs ten pounds, and is really a round ball. Now he is happy. He takes his paper and pencil, draws the round ball, puts a 10 inside it, paints it red in his mind's eye, and works out the problem. When he brings me the result, I inquire at what point the redness of the ball came into his calculations. He looks through them and finds it didn't come in at all. The result would have been the same for a blue ball. Then I ask him where the 10 pounds came in. He looks again and finds he did not use it; or, if he did, it cancelled out, so that the result would have been the same for a fifty pound ball. Finally, I ask him where the roundness came in, and he found he did not use that at all. So I say to him, "Don't ask me for a lot of unnecessary things again." But I think I hear you sympathizing with the poor student. "What harm," you say, "did the redness of the ball do? Why did he sin in thinking it was ten pounds in weight and that it was round, if after all, these things did not matter?" Well, I agree that in this particular case the redness did no harm. But I suspect that if I let the student think the ball is red he will come to me some day with some ideas founded purely upon the redness of the ball. He will be troubled because he will want some other ball to do the same sort of thing that this ball did, and will be unable to satisfy himself because, perchance, the second ball is blue. Then, I shall have to go to the trouble of raking up past history to show that the redness did not matter; but, if he has enjoyed the vision of redness for a long time, his whole mental equilibrium may be destroyed if I take it away.

We frequently delude ourselves into the belief that we have a model when in reality we have no such thing. Thus, for example, consider the case of the Bohr theory according to

which we think of the atom as a sort of solar system. One who thinks he has a model in the Bohr Atom, because what he draws for the atom on paper looks something like the solar system, is in the position of one who deludes himself in a manner which I may illustrate as follows:

Suppose I should see a strange new being, a giant, and exclaim, "See what huge muscles he has, such a man should be able to swing a hundred-pound hammer with ease. See what large eyes he has. Such a man should gather enough light from the skies to see the stars invisible in our most powerful telescopes. See how far apart his eyes are. Such a man should have steoroscopic vision which would enable him to see depths in mountains hundreds of miles away, and so on." And then suppose when we came to investigate this creature we found the weights which he did lift had no relation whatever to the size of his muscles, but depended in a rather peculiar way upon the length of his hair and the color of his eye-balls. Suppose that his powers of vision were no greater than ours, and such as they were were determined by the difference between the lengths of his toes and the diameter of his ears. And suppose that far from having stereoscopic vision so superior to ours he could not see depth in anything at all. Nobody can deny me such satisfaction as I might get by continuing to think of this monstrosity as a man, but I think I should have to admit that he was a very peculiar kind of being, a being so different from other men that it would be very unsafe to utilize my experience of such men to come to a conclusion as to what he would be likely to do. Such experience of him as I might develop would have to be founded entirely upon his own actions. In the sense that this strange being represents to us the model of a man, the old Bohr theory may be said to represent a model of an atom. But after all, it is the properties of the thing that we study which count. It is of these that we make use. The great thing is to get our minds going in the matter of thinking about these things; and, for some peculiar reason inherent in the psychology of mankind we think much better if we have a model than if we have not. I may even go farther, and say

that we think much better if we think we have a model even though we have not. The valuable work of stimulation to think about the things of nature gets largely done before we come to the terrible realization of the fact that we may not, in our mental process, have utilized at all any of the features of our supposed model. This model is a deceitful thing, but it is well gowned and adorns well the drawing rooms of our brain provided that we do not listen too much to its philosophy.

Professor Johnson asked Professor E. M. Patterson whether he thought the forecast of Sir George Paisch that five years from now there would be world wide free trade, was likely to come true.

To this Professor Patterson replied: "In my judgment there will be a tendency in the near future toward lower tariffs and the elimination or modification of many other interferences with international trade. It is not probable however, that anything approximating complete free trade will come in the near future if ever."

Professor Patterson answered a question of Professor Millikan's about the desirability of any given country, such as the United States, endeavoring to protect itself against what is known as "dumping." He pointed out that a distinction can be drawn in principle between the ordinary type of protection and those devices which are employed to prevent dumping. Dumping is to be defined for the purpose as the sale of goods in one market at a lower price than in another, the price usually being lower in the foreign market than in the domestic. Dumping is usually sporadic. In practise it is not easy to detect, but many countries have devices which are intended to prevent it. It is quite possible however for a country to employ these devices with considerable effect while at the same time maintaining a general free trade policy.

Drs. Conklin, Cheyney and Bazzoni also took part in the discussion.

INDIA'S WILL TO BE A NATION

By W. NORMAN BROWN

(Read April 24, 1931)

INDIA to-day offers a spectacle that might have confounded some of the learned commentators of forty years or more ago, who, like Sir John Strachey, expressed the opinion that the first and most essential fact to be learned about India is that there is no such country. This opinion still has currency, and books on India continue to express it with variations, pointing to linguistic, racial, religious, social, and political differences as centrifugal forces rendering nugatory any hope of national unity.

That the differences exist is undeniable. India's population contains representatives of the white-skinned, the black-skinned, and the yellow-skinned races; its languages are of four major families of speech, each exemplified by few or many languages and dialects; of religions the two great families of Indic and Semitic are numerously represented, a third, the Persian (through Zoroastrianism) has a few adherents, and primitive faiths have a large following. In the social order there exist important separatist tendencies especially among the Hindus, who have developed the caste system and the "joint-family" system of economic life. Politically, India is now divided between two types of administrative entities, which daily grow farther apart, namely, the bureaucratic system of British India, now being converted into a representative system, and the autocratic system of the Indian states. Viewed theoretically, with a side glance toward the history of nationality in other countries, these differences might logically preclude the attainment of nationality in India. There seems to be ground for the opinion that there has never before been a manifestation of nationality in India, unless it be the regional Hindu counterblast to Mohammedan

domination led by the Maratha chieftain Sivaji in the last part of the seventeenth century. Yet the fact remains that to-day a national spirit exists in India, already of some strength and ever growing stronger, and any worthwhile appraisal of it must be based upon observation of it itself rather than of nationalistic phenomena elsewhere.

At this point it is fitting that I should indicate the meaning I am attaching to "nationality." By that term I mean the *will* of a people to constitute itself a nation. And by "nation" I mean a state composed of people with common traditions and ideals, as distinguished from a mere "state," which may consist of peoples mutually uncongenial, as in the old Austro-Hungarian empire. Nationality is a spiritual thing and its goal is nationhood; for political unity is necessary to preserve nationality. Until that goal is won it is an ideal, as it was until comparatively recently with the Poles. Once the goal is attained, nationality becomes a sentiment, and the nation finds other ideals, such as imperialism.

The present unrest in India is a manifestation of nationality struggling to produce a nation. Although it may have begun following the Sepoy Mutiny in 1857–8, partly in protest against wanton acts accompanying suppression of the Mutiny, and partly as the effort of a small dissatisfied group from the middle classes to win some measure of control over their country's affairs, it has now in about seventy years' time enlisted a sufficiently large number of the middle classes, supported more or less intelligently by the lower classes, to justify the use of the word "general." Intellectuals of widely varying shades of political opinion, such as the very moderate Sir Tej Bahadur Sapru, or the more pronounced Srinivasa Iyengar and Madan Mohan Malaviya, or the extreme Gandhi, Vallabhbhai Patel, and Jawahar Lal Nehru, or the still more radical Subhash Chandra Bose, all in one form or another support nationality. The movement has both negative and positive purposes and reasons for existence; there are aspects of India's general life both to help it and to hinder; it has a place in the logical development of Indian history, and is to be

understood in the light of the past quite as much as of the present.

It is presumably not necessary to recapitulate the many important indications of strength lying in the movement; for I am concerned here not so much with the recent happenings it has motivated as with its basis in wider Indian culture. Let me merely remind you that during a period of about forty years, from about 1880 on, after the pax Britannica had been firmly established in the land, it slowly won measures of self-government from the British parliament, increased share in the civil, education, military, and other services, and in 1919 obtained a constitution which allocated to representatives of a limited Indian electorate rights that forty years earlier would have been considered visionary. Recently it was able to oppose the parliamentary commission on Indian statutory reform, headed by Sir John Simon, with such effect that the work of the commission was shelved quietly and a Round Table Conference of Indian princes, liberal nationalists (members of the Indian National Congress refused to participate), and representatives of the British government, was called in London last fall to draft a new scheme, which it was hoped would be acceptable to Indians. Even this conference failed to win the approval of the Indian National Congress, the strongest nationalist body, and now a new conference is to be called between representatives of his Majesty's government and Mr. Gandhi, as leader of the Congress, the "seditious saint," as Mr. Churchhill calls him, who only a few months ago was confined in jail. Without delaying over the history of the movement, I mean to pass to a consideration of what seem to me the more important aspects of Indian nationality, namely, its basis in the life of India to-day and its larger historical significance.

It is almost an axiom that nationality first comes into existence when a people is subject to foreign domination. If we look at Poland, Bohemia, Ireland, Arabia, and many other countries, we find that the immediate stimulus has been foreign control, and India is no exception. It is the fact of

British control that has aroused India to self-expression, and the first objective of nationalism there is the negative one of removing foreign domination. The irritation results basically from the fact that the control is foreign, rather than that it is harmful to the country, although arguments to the latter effect are elaborately developed, with greater or less justice, and constitute the bulk of nationalist propaganda.

It would be unprofitable to enter here upon the highly controversial subject of the morality of the British occupation of India; that question has been discussed often. But a few points may be indicated in connection with it. The relation of India to Britain is that of a *colonie d'exploitation* not of a *colonie de peuplement*, as are Canada, Australia, New Zealand, Africa; and the natural consequence of this fact is to prevent the rise of understanding between the British temporary residents and the Indians. Further, the educational system of India has been concentrated upon British culture rather than upon Indian, using English as the medium of instruction, to the great detriment of the instruction, and this was another point bound to result in unfriendliness. Thirdly, the Englishman, often without any intent, manages to convey to others a feeling of racial superiority, and this trait, too, with its many extensions and ramifications, has disaffected Indians. But no matter what imperialism had been holding sway, the growth of national feeling was inevitable. The intrusion of a foreign culture supported by political supremacy and arrogating to itself in consequence a superiority in morals, literature, art, and other phases of life arouses in a subject, but cultured, people an affirmation of the worth of their own institutions and of themselves. Whether the British imperialism were "satanic," as Gandhi characterizes it, or a source of light and prosperity—to India as well as to Britain—as its proponents maintain, the present resentment was unavoidable. Freedom is a natural desire of the human heart, and the Indians are human. We may accept, then, the antipathy to the British rule as natural, a "given" element in the situation, and without stopping to analyze and appraise it pass on to topics less certain.

To attain enduring strength nationality must have more positive nourishment than hatred of foreign rule. It must be based upon a genuine national culture, unified and vital, that has powerful common ideals, guaranteeing the ability to achieve unified action after as well as before securing political independence. That some such basis exists in India is antecedently indicated by the past history of the country. Recent archæological exploration in the Indus valley, first published in 1924, has shown existing there a highly developed civilization as early as the beginning of the third millennium B.C. It may be even older, for the earliest stratum is below the seepage level of the Indus and, because expensive caisson excavation would be required, has not yet been revealed. The interpretation of the civilization there discovered is as yet uncertain, chiefly because it is still impossible to read the writing it has given us, yet the finds assure us of a high antiquity for India's culture. Nor have we yet been able to relate that culture satisfactorily with the later culture in the rest of India, and, conversely, we are ignorant of the beginnings of much of that later culture, as, in particular, the state of civilization in India before the Aryan invasion during the second millennium B.C., and the antiquity of culture in the Ganges valley, where again we have not reached the lowest archæological stratum. But from the early part of the first millennium B.C. we have had a stream of culture in India, continuing unbroken down to the present, although with numerous vicissitudes and modifications. Few other regions can present such a history; certainly not Egypt and Mesopotamia; possibly only China.

It is this continuity of native Indian culture that is its most striking characteristic. It has, for one thing, withstood the assaults of numerous invading barbarians. Possibly the earliest of these were the Aryans, although, as I have remarked, we cannot be sure what their relationship is to the whole subject of civilization in India. At later times the Sakas (Scythians), the Huns, and others burst upon the country, to destroy but in the end to be assimilated by native

Indian society and to adopt Indian institutions. This assimilation is not in itself especially surprising; for Rome in the same way conquered her uncivilized conquerors. But a more critical test came when Indic culture was brought into conflict with other highly developed cultures, and still maintained its vitality. This has occurred three times.

The first of these tests extended from the fourth century B.C. to the fifth century A.D., when India was brought into touch with the Hellenistic culture springing from a union of Greek with Persian and Bactrian, after the conquests of Alexander, who left India 325 B.C. Northwestern India was for some centuries strongly Hellenized, and the influence was felt in central and even eastern India. We see such results as that the royal Mauryan palace of the emperor Asoka built in the third century B.C. at Pāṭaliputra (modern Patna in Bihar) was modeled on an Achæmenian palace at Persepolis, that this same monarch had his edicts carved on rock in different parts of India, in imitation of the earlier inscriptions at Bahistān, or on pillars which exhibit some western elements in motif, style, and technique. One distinguished scholar, the late Dr. D. B. Spooner, has ingeniously elaborated a theory of a Persian period in Indian history and has assumed Persian descent for the Buddha, although his arguments have failed to win acceptance. Under this same Hellenistic influence the art of northwestern India and the general "Gandhāra" region during the first century preceding the Christian era and the four following was strongly marked with Greek elements and spirit, showing naturalism, a repose in the figures different from the tension of the native Indian, and the use of Grecian iconographic types and ornament. The Greek language was in use on coins side by side with Indian tongues; literature even shows Grecian influence. Science, especially mathematics and astronomy, borrowed freely. Yet in the end India rejected Hellenism, as has been convincingly demonstrated in the field of art by Dr. Coomaraswamy. Hellenism succumbed before the onslaught of the Huns in the fifth century A.D. and never again got hold. India took unto

herself neither Greek philosophy nor Greek sculpture. The Buddha image of India proper, long thought by Foucher and other distinguished scholars to be a development out of Hellenistic types in Gandhāra, now appears to be derived from an indigenous iconography of Yakṣas (vegetation divinities) and to contain only a few unimportant accessory Grecian elements. Although continuing for many centuries, the Gandhāran, so-called "Indo-Greek," art took no hold upon the folk of India proper, who continued the orderly development of their own ideas and style. They let the legions of western thought and plastic form as well as of warriors thunder past and plunged again into their own philosophy and art. Traces of Greek influence appear in more than one department of India's civilization, but they are relatively inconsequential; if I may venture a comparison that is bound to be inexact, they are less apparent or important than are the Arabic and Saracenic in European literature, art, science, mathematics. Opinion as to whether Indic or Greek culture is the superior may be varyingly determined by the environmental prejudices of the eastern or western observer, but no one can claim that in the test the Indic has proved less strong.

The second powerful culture with which that of India has come into conflict is the Islamic, promoted chiefly by Central Asiatic peoples who had been converted before reaching India, and the contest between the two is still in progress, providing the major internal political problem of India. The first Mohammedan incursions took place in the eighth century, but the struggle became acute at almost exactly 1000 A.D. Every part of India sooner or later fell under Mohammedan control, but the greatest cultural influence is seen in the northwest, where the Mohammedans entered, in the west, and in the east. The central and southern parts are least affected. Not only is Islamic culture strong; it has also been propagated with the aid of political domination to a degree which the Hellenistic never was. It has a physical power to support its philosophy, religion, art, law, literature, and social order that students of European history know well.

They have seen its advances in the west, stopped only by Charles Martel at Tours, in the east reaching up to Vienna, in the north coming down into Russia, in the south at one time dominating the eastern Mediterranean. We may wonder, perhaps unprofitably, if Europe with its Greco-Roman-Christian culture, had she been conquered politically by Islamic peoples, would have been able to resist Islamic civilization, as India has been able to do when so conquered.

It is one of the characteristics of Mohammedanism—as of the two other great Semitic religions, Judaism and Christianity—to tolerate no rival, no different faith. It is thus an aggressively proselyting religion, unlike Indic religions, especially Hinduism, which has no care what intellectual opinions a man may hold provided his social acts conform to its standards. Mohammedan intolerance in India, supported by political supremacy, took an extreme form. Temples were desecrated, sacked, demolished; books were destroyed; priests were killed; conversions, symbolized by circumscision, were effected, non-violently if possible, often by force. The Mohammedans gave the death blow to Buddhism in India, already declining before Hinduism, and seriously weakened Jainism. There were at certain periods areas in which from the Hindu (native Indic) point of view civilization seemed to have perished from the earth. Yet Hindu civilization did not perish. It was beaten down in some sections or driven out; in others, at the farthest limit of Mohammedan power, it was hardly affected. To-day three-fourths of India's approximately three hundred and fifty millions are counted Hindu and one-fourth Mohammedan, and the Hindu population through its superior intellectual status has overcome the greater physical aggressiveness of the Mohammedan, and holds the advantage.

The integrity of Islamic culture in India has, indeed, been touched by the indigenous Hindu. To the immediate observation of the eye this fact is observable most easily in architecture. In the early period of Mohammedan conquest mosques were built in western Asiatic styles brought from Persia or even Egypt, but Indian elements soon were adopted,

and "Indo-Mohammedan" architecture, especially from the fifteenth century onward, although conforming in purpose and general design to the western Islamic, uses much of decorative motif and structural technique that is Indic. Less apparent on the surface is the fact that Islamic philosophy in India contains much of the mysticism sympathetic to the Hindu temperament, although the immediate source is often likely to be Persian Sufiism. In religious practises there exists at times a measure of rapprochement, as when certain Mohammedan groups observe Hindu caste regulations, or Hindus assist in the Moharram celebrations. Some Mohammedan reform sects show the influence of Hindu notions, as in asserting the human incarnation of a deity. Many Mohammedans in their personal life apply in some measure the Hindu doctrine of Ahiṅsā (the non-injury of animal life, lower and higher) toward the rest of animate creation. Mohammedans are coming to feel that they as well as the Hindus are heritors of India's historic culture, as in the Hyderabad state, where an autocratic Mohammedan government is sedulously conserving and publishing the great series of Buddhist, Hindu, and Jain Caves at Ajanta and Elura.

The ideal goal of Hindu culture in regard to Islamic culture would be to push it out or emasculate and absorb it as it did the Hellenistic. No such event is in immediate sight, although it is evident that the position of Hinduism is now far better than it was two centuries ago. With the breaking up of the Mughal empire during the first part of the eighteenth century the really important political support was withdrawn from Islam, and as the British hold became firmer, aggression by force for religious ends ceased, with the result that Hinduism has gained ground. Sikhism, a compromise religion between Hinduism and Buddhism that arose about 1500, is turning steadily toward Hinduism. The present nationalist movement is as truly a reassertion of Hindu culture gainst the Islamic as it is against the European, although this aspect of the movement is less consciously perceived either in India or outside. The immediate political struggle against British rule

withdraws the full attention of many observers and many nationalist political leaders, like Gandhi, who sincerely seems to believe that Hinduism and Mohammedanism can exist peaceably side by side without the one gaining an advantage over the other. He is, it seems to me, mistaken, and it is noteworthy that he sees this solution as possible provided the Mohammedans accept Ahiṅsā. Mohammedanism had its chance to overwhelm Hinduism and failed; Hinduism, without being fully aware of the implications and logic of its present activity, is now making an effort to get back what it lost from the eleventh to the eighteenth century.

The third great culture which has come into India is the European. Had it come in from the northwest as did the Islamic, these two might have nullified each other; but it came in from the seaports, chiefly Calcutta, Madras, and Bombay, and instead of being a rival to the Islamic, served with it to enfilade the Hindu. Like the Islamic it has been assisted in its progress by the political domination of its proponents, but without the same aggressive use of force in its favor. Yet the extent to which western culture has made its way in India is great. The chance traveller may see evidences in architecture, as in public buildings in all parts of British India constructed in European styles, to the neglect of Indian, railway stations in Gothic, colleges in Gothic, even private houses in renaissance. The economist knows how the Indian handicraft products have to large degree been displaced by western machine-made goods and how the financial policy of India has been subordinated to that of Britain. Going more deeply into national life was the fixing of English as the official language and as the medium of instruction in government high schools and colleges, and the devising of a system of higher education which concentrated on English history, European philosophy, English literature, and neglected the languages, literature, and historic culture of India. This system had the natural result that the students of the colleges learned nothing about India and very little about the foreign civilization from which their cultural study material was

drawn. The motives of the British who imposed upon India this sort of education and European architecture and industrial products need not be discussed; we are concerned only with the results. Western religion was also introduced, and had a prestige from being the religion of the ruling race although it was not promulgated by official means. Many of those who in the nineteenth century clamored for European ideas and institutions were high-minded Indians, like Ram Mohan Roy (1772–1833), who were captivated by the success the Europeans had won, thanks often to industrialism, and thought that all India's woes would automatically cease if she were westernized.

The vigor of the western cultural attack upon India has been great, being supported not only by the prestige deriving from the political superiority of its proponents but also by the material superiority of the accompanying industrial civilization. Yet again the indigenous Hindu culture has resisted it as it resisted the Islamic, and is now asserting itself against it. Every visitor to Delhi to-day can see the expensive buildings recently erected there as the new capitol, in which only a half of the style is European, the rest being adapted from Indian, so that the resulting architecture is a kind of bastard Eurasian by Rome out of Sanchi or Mathura. But now in some places buildings have been erected at government expense that are almost entirely Indian; so much has the contemporary Indian renaissance accomplished in that field. In the colleges Indian subjects have partly displaced European, and by some the vernacular languages are admitted toward the absolving of matriculation requirements and as the medium of instruction in some high schools. Hinduism has mustered its strength against Christianity: reform movements, like the Arya Samaj, oppose it as well as Islam, and organized Hinduism makes efforts to hold the depressed classes, which on account of their debased social and economic situation have provided the best field for Christian enterprise. Christianity itself in India has sometimes had to compromise with Hinduism, as when Roman Catholic churches have made

allowances for the caste distinctions of converts. The present nationalism directs its greatest energy against the political domination of Britain, but I need not remind you of the campaign against westernism in the wider sense, which is preached, for example, by Gandhi.

Nationality in India to-day is predominantly a Hindu phenomenon, and the cultural renaissance accompanying it in literature, painting, and music is also Hindu. The number of Mohammedans and Christians supporting it or sharing in the renaissance is comparatively small; by far the greater part of each of those two groups is outside the nationalist movement. Efforts have been made to bring them in, especially the Mohammedans, for it is realized on all sides that the success of the nationalist movement for home rule is seriously prejudiced by the perpetual conflict between Hindu and Mohammedan, but no permanent results have been achieved. As I write these words, the newspapers again contain dispatches of the effort and failure of Gandhi to bring the two communities into harmony. The nearest approach to union came in 1920–21, at the time of the Non-coöperation movement, when Gandhi espoused the Khilafat cause, but the results achieved were of no permanence. That cause was of no vital interest to Indians, even the Mohammedans, and was not the solvent that could unite the oil and water of the two groups. Far more weighty was the fact that the Hindus outnumber the Mohammedans three to one, and that, as the franchise was extended by the reforms, the power of the larger group was potentially and actually increased and the situation of the smaller group was seriously threatened. The question of communal representation overshadowed all else, and the two groups came to an agreement concerning the proportion of seats in the various legislative bodies each should have, a solution quite unsatisfactory to the Hindus and therefore not calculated to remove the nervousness of the weaker Mohammedan group. At the time of the Non-coöperation movement there were many Indian Christians who supported it, and some Indian Christian groups are to-day supporters

of the National Congress party, but the community in general seems anti-nationalist. Its members begin to realize that the success of nationalism would remove the advantage they have at present in the economic struggle as the co-religionists of the dominant race. Gandhi, prophet of nationality, wooes the Christians as well as the Moslems, but without appreciable success.

The enduring nature of Indic culture is much easier to demonstrate than is its inner source. Somewhere there is a mystical spiritual quality that animates Indian civilization, that constitutes its life principle, without which that civilization would have succumbed to some one of the three other cultures it has faced, the element that gives ground for belief that such a thing as Indian nationality exists. What that spiritual quality is I know not any more than I know what life itself is; what it feeds upon we may perhaps be able in some measure to indicate.

The underlying unities of nationality are, I believe, generally considered to be those of territory, race, language, religion with philosophy and ethics, art, literature, law, social institutions, history, political organization (government) either antecedent or prospective, economic interests. It is rare that all of these are present in any single community to nurture nationality, while in different communities their relative importance may vary. Somewhat mechanically, may we run through this list, pausing on those that seem to require comment.

The geographical unity of India is so well known and so apparent from the briefest glance at a map that I will do no more than direct your attention to the fact that the country is surrounded on two sides by ocean, on another by difficult mountains, and on the fourth by desert. It has a high degree of isolation, and the only irruptions of consequence have all come from the northwest and west, except in recent centuries when the Europeans came by sea. In modern times, under British rule, the increased facility of communication between various parts of the country has made it more nearly than

before a territorial unit, although some few sections are still awkward to reach.

Racially, India is by no means unified, as I have already noted, and the disunity is sometimes offered as prejudicial to nationality. The effect of this disunity is, I believe, exaggerated. May I venture the suggestion, subject to correction from sociologists and anthropologists, that the only point of racial difference tending seriously to produce race antipathy is that of color. Anthropometric variations have little, if any, weight in that connection. It is pigmentation that renders black and yellow uncongenial to white in America to-day and that makes black feared and despised by white in Africa. In ancient India there was at one time a similar situation. The invading Aryans, being light, drew a color line between themselves and the dark-skinned people already there. Unions, both regular by hypergamy and irregular, usually between women of the dark, conquered folk and the men of the conquering, made the color line ever more shadowy, until to-day Indians are all dark, but varying in degree from mere brunet to very black, sometimes within the same subcaste. Except where caste and color roughly happen to coincide, the feeling for color distinction is somewhat akin in quality to that existing among mulattoes in America, where it is considered a step down for a light girl to marry a dark man. The Indian himself, unacquainted with the science of anthropology, has now for some thousands of years been unconscious of differences of race in his country; he has recognized only color gradations within the same race. It is true that with the recent arrival of anthropological knowledge there has arisen a certain "Dravidian-consciousness" in South India, being anti-Aryan in tendency, but this has so far not been strong enough to interfere with nationality.

A genuine racial question has been born within the past two centuries, perhaps even more recently, and it has been born in consequence of the arrival of the British. The question is acute not only in India, but also in other British Colonies, especially in Kenya, Africa. If it had been the

French, with their easier attitude toward darker folk, who had been the victors in the fight with the British for India during the eighteenth century, this racial problem might not be an issue now. But the present racial problem is one that, instead of hindering the progress of nationality, fans its flames. Commentators like Mr. C. F. Andrews and Lieutenant-Colonel Osborne consider it the most important element in the development of nationalistic sentiment; for they find the aloofness and reserve for which the Englishman is notorious in Europe magnified in India to a point of assumed superiority culminating in physical brutality, where a sensitive people cannot tolerate it.

The linguistic differences, so numerous and profound, again loom much larger in the eyes of western observers as an obstruction to nationality in India than is justified by actualities. Certain speech groups, it is true, such as the Telugu, Sindhi, Kanarese, have intimated a desire for separate recognition, but the demand has not yet shown pronounced strength except in Sindh where administrative considerations weigh heavily. The masses are, of course, not conscious of the fact that their country is divided between different speech families. To them there is only a difference of language or dialect, as to the French peasant there is a difference between German and French or to the English countryman the speech of Devon differs from that of Northumberland. But the important point in India in connection with the effect of language upon nationality is that native India, that is Hindu India, in which as I have indicated the national movement has its life, has for twenty-five hundred years, possibly longer, had a common language for the communication of ideas throughout the upper levels of society. This is, of course, the cultured literary language Sanskrit, and with it in some religious environments the kindred dialects of Pali (among Buddhists) and Ardhamāgadhī and Māhārāshṭrī Prakrit (among Jains). This fact, so well known to Indologists as to need to demonstration, perhaps should be called to the attention of political theorists. By means of Sanskrit educated men in ancient

India could always communicate with one another, whether from Kashmir, Bengal, or Travancore, and until the Mohammedans brought in Persian and Arabic it was the unrivalled cultural language of all India, the homogeneous blue water of the ocean covering the variegated floor of popular speech below. It still remains a general cultural medium among the Hindu portion of India's population, three-fourths of the whole, in those limited groups which adhere to the traditional lore. We are reminded of mediæval and renaissance Europe, when, for example, Queen Elizabeth could speak in Latin—and with fluency and vituperative force—to an ambassador from Poland.

But the analogy of Latin in Europe is incomplete. The position of Sanskrit in India was even more dominating. Not only was it the language of religion and religious literature—of the church—it was also the chosen vehicle for philosophy, for belles lettres, for poetry, drama, story, for law codes and treatises on state craft, for mathematics, astronomy, astrology, for manuals of art and architecture, of natural science, of medicine. It covered the entire field of human intellectual activity and stunted the growth of popular speech as literary media. It was used not only for writing but also for vocal discussion among the learned, and still is so used among the traditionally educated. It held, in short, a linguistic monopoly of civilization. It gave to India a unity of language that to a large extent compensated for the disunity of local speech, and to some degree still does so.

In recent years a new means of linguistic unity has been found in English. This language, so much resented by nationalists, some proposing Hindi or Hindustani as a general language for India, is now the common speech of those educated along western lines in the modern schools and colleges. There appears to be no likelihood that its importance in matters of "practical" life will diminish; rather it will increase. As western science and ideology spread in India—and they are bound to do so—the medium by which they are taught becomes increasingly popular, and that too at the

expense of Sanskrit, which tends to be restricted to philosophy, religion, and literature as art. English has an additional advantage over Sanskrit in that through it Hindu and Mohammedan can communicate easily. It, and the scientific and political ideas brought from the West under its ægis, provide one of the chief aids for bringing together those two antipathetic communities and thus for overcoming the most important of all the elements of national disunity in India to-day.

There is also a phase of the social life in India, the caste system of the Hindus, which is sometimes thought to be more antagonistic to nationality than I believe warranted. Caste is undoubtedly partly separatist in its effect. It stratifies Hindu society into mutually exclusive groups, and thirty years ago it was thought by many experienced students, like Risley, that political parties would develop along caste lines. In most of India this has not been the case; the caste lines have been horizontal not vertical; for the castes deal with social rather than political matters; but in south India, where caste disabilities are the most onerous, the alignment is of Brahmans against non-Brahmans, the latter including all the unprivileged groups, yet transcending the caste divisions among them. Here the development of nationality is slower than in other parts of India; it is waiting upon the attainment of domestic balance.

The disintegrating tendencies of caste are partly obviated by two general conditions. First, the institution itself is becoming far less strict. Caste is an order of society developed through millennia in a settled economic environment that had the self-contained village as its unit. That economic environment has been altered during the past hundred years, and the alteration is being accelerated. Instead of being self-contained, the village now draws much from outside. Some of its old handicraft industries have been reduced by the influx of machine-made goods. Railroads, good highways have made communication much easier, and the village is more than ever a part of the world. Industrialism, too, has come to

India, and while the population of the country is still to about ninety per cent contained in the villages, an urban industrial population is also coming into existence. With the alteration of the economic basis of caste, the system itself is gradually being disorganized. Old restrictions concerning the preparation of food and other objects of life and concerning association with members of other castes have had to be eased as more and more articles have been brought in from outside and as railroad travel has developed. Further, the intellectuals of India are calling for reform that reduces the strictness of caste regulations. The sanctions for the system that came to be vested in religion are attacked by some who, like Gandhi or the Arya Samajists, find them later developments of Hinduism, not of the earliest stratum of the religion, or by others, influenced by western science or philosophy, who deny their validity entirely. While caste does not yet show any prospect of extinction, it at least shows much modification, and this, in connection with nationalism, is great enough to indicate a marked weakening of its disunifying character.

And, secondly, there are respects in which caste contributes to nationality. It provides disciplined groups accustomed to a certain amount of unified action. It is also a special feature of Hindu culture, intimately associated with the Hindu religion, and as such may be used by the nationalists to provide propaganda material when they call upon Hindus to defend home and institutions from the aggression of an alien culture.

If race, language, and the social order of Hindu India only vary in their effect upon nationality from being slightly inimical to being neutral or slightly favorable, we find in religion and the metaphysics connected with it an element of great positive value for nationality. Underlying the numerous faiths and sects native to India there are certain common attitudes of mind, general principles, which might be called metaphysical axioms, and it is the common acceptance of these that gives the greatest strength to national unity. These I shall mention, although there is no need to discuss them at any length.

The first is an idealism, a mysticism that denies the worth, and in some cases the final reality, of the phenomenal universe, including even the gods, and any experience in it, and admits reality only to soul. With some this belief asserts One Existent Soul, that is, it becomes pantheistic or monistic, and this is the prevailing belief of Brahmanic Hinduism and the Vedanta; with others it may affirm the pluralistic belief in innumerable souls, as in Jainism and the Sāṁkhya philosophy; with still others it may deny soul entirely yet admit a renewal of existence after each death, as with Hīnayāna Buddhism. Accompanying this first notion is the second, a belief in rebirth, the passage of the soul or of consciousness at death from one sort of body to another through innumerable lives, the soul (or other transmigrating element) being bound to (real or false) matter through ignorance, and the bondage inevitably being painful. The character of each new existence is determined by the next idea, namely, the action of Karma (deeds performed with emotional coloring) constituting a cosmic force that operates objectively and inescapably to produce as in a mathematical equation a result exactly commensurate with the deeds. Finally, salvation, escape of the soul from this sorrow-laden round of rebirth, comes by ceasing to perform deeds with any emotional content, that is, by killing Karma, and the chief means of accomplishing this are generally recognized either in theory or in practise as being the attainment of mystic knowledge, or the performance of good works, or devotion to some saving deity, all which means are materially aided by observing the ethical principle of Ahiṅsā, non-injury of creatures (Gandhi's "non-violence") and self-subjugation by asceticism more or less severe.

These notions inform the intellectual side of Hindu culture. Religion, law, art, social and political theory are integrated by them. In themselves they constitute the basis of religion, and the variant theological, ethical, metaphysical, and psychological additions to them in Hinduism, Jainism, Buddhism are of secondary importance. Law revolves about them; the social order of caste is sanctioned by the application of Karma

—each man has been born to precisely the state demanded by his previous deeds. Art, reflecting the idealism of philosophy, finds the life of the mind more real than that of the flesh, and therefore does not depict nature but imagination, and the struggle in the round of rebirth is reflected in the tension that marks all Hindu iconography.

Most strongly is this condition apparent in religion. Hinduism is now almost without a native Indian rival; its contest is with the foreign Mohammedanism and Christianity. With only minor variations it extends over the entire country; its once great rivals, Buddhism and Jainism, are the one exiled from India, the other almost withered away. Once they contested with Hinduism and won many millions of followers; to-day the descendants of those followers are almost all back in the Hindu fold. No attack yet directed against Hinduism has shaken it. Islam, Christianity, atheism within its own ranks to-day have not affected it. This religion, with its common literature, common deities, common art, common ideas, is the greatest unifying factor of India, the cement that holds it together. Roughly it claims three-fourths of the total population, and where they feel their faith imperilled they are one.

The hold of the ideals of religion upon the people is aided, to a degree unusual elsewhere, by the literature in which they are enshrined. This is not, for the people as a whole, the Rig Veda and the Upanishads or the treatises of Śankarācārya or Rāmānuja, but the great epics, the Mahābhārata and the Rāmāyaṇa, and the Purāṇas, read by the learned, heard in recitation by the illiterate. In these are found the stories of Rāma, the national ideal of manhood, and of Sītā, the ideal of womanhood, of Krishna, God made man, of all the figures that touch the national imagination. These figures are of the soil of India itself; their lives moved in India; their names are associated with India's mountains, rivers, and cities. Their tales are told with Indian phraseology and with Indian figures of speech. We in the West can scarcely understand the strength of the hold these heroes have upon the folk. Our

religious heroes are not our national heroes; they are importations into Europe and America. They move in a strange environment; their ways were not as ours; the language in which they are described brings no primary response in our hearts. Religious literature and epic literature with us are dissociated; in India they are welded into the closest unity.

Hence in religion lies the prime basis of Indian nationality, and we see why several reform sects have arisen that preach a religious nationalism against the actively proselytizing Islam and Christianity. From this fact also springs the phenomenon that the most powerful political leader of the land is at heart a religious. The life of sainthood which he embodies is a national ideal; the book which he quotes with the greatest effect is the Bhagavad Gītā, India's best-loved work of religious inspiration. India does not lean toward government separated from religion like the present British administration; she does not yet understand such a division; to India as a whole patriotism and religion are still inseparable. Hinduism is not yet a political entity, as is Islam in India, for it needs to learn organization; but it gives signs that it may become one.

Great as is the strength of religion in India through the hold it has upon the people, Hinduism has an additional power in its ability to absorb ideas. It has always been fluid—a fact which, again, is more familiar to Indologists than to students of politics. Its final victory over Buddhism and the now almost complete victory over Jainism have not been due entirely to the previous grip it had upon Indians but also in part to the capacity it had to compromise. The leading ethical ideal of these two rival faiths, Ahiṅsā, was accepted by Hinduism and has become the corner-stone of its own ethics. The Buddha is recognized as one of the *avatāras* (incarnations) of the god Vishnu. In modern times reform sects of Hinduism have admitted Mohammed and Christ to the rank of great teachers; Gandhi is only one of many devout Hindus who read the scriptures of other faiths. There is in this respect an amazingly broad tolerance in Hinduism that suggests a possible means by which Hinduism may reabsorb the de-

scendants of all those Indians now numbered as Mohammedans and Christians—slight commingling already occurs—it may take them all in by giving some sort of recognition to the leading ideas and the great prophets of the invading religions. The present modification of caste restrictions is one of the best evidences we have at this time of Hinduism's adaptability to changing circumstances.

We have mentioned above in our list of subjects potentially providing a unity helpful to nationality the matter of common history. In this respect it used to be said that India was without any historic sense. There are few works of history in her abundant literature, and most of these make little distinction between fairy-tale and fact. Our judgment to-day must be modified. The professed historical works, unsatisfactory as they are, nevertheless are in better standing with scholars than they were thirty years ago. More important for nationality is the fact that a historic sense is growing in India and growing in a very nationalistic direction. The source is largely in the results of archæological exploration; hence the Archæological Survey of India is one of the best liked divisions of governmental activity. Excavation and study have been revealing to Indians the extent, quality, and age of their ancient civilization and its influence upon the rest of Asia. National imagination and pride both are stimulated by the discoveries, which show that India has a culture scientifically demonstrable to rival those of other regions and in some points to surpass them. The natural consequence is an increase in national self-confidence.

As religion provides nationality with its greatest strength, so too it is responsible for its greatest weakness. This weakness does not lie in any quality of Hinduism itself, which is the faith of the group that is now nationally minded, but in the antagonism religion inspires between Hindus and Mohammedans. Approximately one-fourth of the population belongs to the latter group, and for reasons which I have indicated it fears the rest. The Hindu-Moslem antipathy is so well known that it needs no discussion here; many observers

claim—and I believe justly—that, if it could be resolved, the attainment of home rule for India would be automatic and immediate. As the years have gone on, the Moslem consciousness, like the Hindu, has been developing nationalistically, although in lesser degree. A scheme proposed by some Mohammedans is that the present India should be divided into two states, the one a Hindu state, the other a Mohammedan state, including in a general way northwestern India and parts of northern and western India, the regions where the Mohammedan element in the population is the greater. This proposition is not viewed favorably by the Hindus, who find the antagonism of the Moslems, at best only latent, at worst actively expressed, an obstacle to their aspirations second only to that coming from the British.

The opposition of Christians to Indian (Hindu) nationality is much less self-conscious than that of the Moslems. Many Christians are still on the fence politically, although the weight seems to be pro-British. The Christian opposition is, in short, only a part of the larger attack by western civilization. This latter operates subtly though the teaching of western science, western political philosophy, whether from Britain or Russia, the spread of western technology and industry, the intrusion of western ways of life, the spur, as we have seen in passing, to much of the nationalistic theory and activity. These things must come to India, and it is here that Hindu culture is being put to the great test of accepting and absorbing what India needs of them without itself being supplanted.

With the growth of representative institutions in India a hindrance to nationality has developed in the country's varying forms of political organization. The government of British India, with the exception of the Northwest Frontier Province, is partly by legislative councils, of whom many members are elected by that portion of the population which has the privilege of the franchise; similar is the case with the central government. But the Indian States, comprising one-third of India's area and one-fifth of her population, are

autocratic monarchies without elected governmental bodies and with treaty rights that preserve them from the control of the central legislative machinery. The princely rulers of these states, opposed to nationality as a middle-class sentiment that threatens their position, are in support of the British power. The idea of a Federated India, as proposed by the Simon Commission and with some modifications proposed by the recent Round Table Conference, has been developed to reconcile the conflicting demands of British India and of the States. Yet the existence of autocracy and democracy side by side is to the nationalists an intolerable contradiction, and some of them state with a frankness that the princes sometimes consider an alarming brutality that these absolute monarchs must relinquish their present rights. The obstruction of the states to nationality is indeed important.

In the economic life of India there is arising another problem for nationality. This life until recently was in general uniform; the country was agricultural, with its handicrafts being only cottage industries supplementing agriculture. As India is adopting industry, that uniformity is being broken. Factory-owner and land-owner find their interests diverging; so too do factory-worker and field-worker. Within the ranks of the present national movement both types of interest are represented; there are leaders who look for India to become a great industrial country; there are others, like Gandhi, with whom it is a religious principle that she should remain agricultural. Of the workers there are some who see their salvation in communism, and do not hesitate to jeer even Gandhi. One of the most influential leaders of the Indian National Congress, Pandit Jawahar Lal Nehru, has considerable familiarity with the Soviet ideas of government, although he works in the closest coöperation with Gandhi, perhaps feeling that the Russian and Indian problems are unlike and that the first step in India is the attainment of home rule. But the differences between the two types of interest are becoming progressively important.

As in the case of Ireland, so with India certain adventitious

outside circumstances have helped nationality. It has, for one thing, received less opposition from Britain than such a movement would have had in 1900. This may sound strange to those who have read that the number of political prisoners at the beginning of 1931 was between fifty and sixty thousand, yet I believe the statement true. India, Sir Joynson Hix has plainly told us, is an outlet for British goods; the yearly trade summaries show that the percentage of British imports into India is steadily diminishing. Two kinds of response to this fact are met in England: one finds India less important as a market and is more willing to let India go her own way; another hopes by methods of mollification to win back the trade. The milder methods of dealing with Indian sedition are also due in part to world-wide idealism in favor of the rights of small nations and subject peoples, so popular during the war and still a slogan in many circles. This idealism, as it exists in Britain, has been an opportunity for India's nationalists.

Another outside help to Indian nationality is found in the increasing respect of the world for India's culture. The "poor, benighted Hindu" is now largely a creature of the past. The examination of India's literature by western scholars, the study of her art as preserved in western museums, the contact with cultured and charming Indians who come in increasing numbers to the West have all aided in building up that respect. Perhaps the most contributory of all was Vivekananda, the hero of the Parliament of Religions at the Chicago World's Fair, who in a few months' time obliterated much of the contempt for Hindu religions that existed especially in America. The award of the Nobel prize for literature to Tagore, the recognition of the scientists Bose and Raman and of the philosopher Radhakrishnan have raised India in the eyes of the general public. India is coming of age in western mentality as well as in Indian. Not only has the West been affected favorably toward India; India herself has had her self-confidence increased by the favorable opinion so aroused.

So much depends upon leadership in the promotion of nationality that we must pause for a moment to take up that point with India. The mere attainment of home rule will by no means complete the aims of Indian nationality. If I am correct in viewing that nationality as a phenomenon essentially Hindu, then the work will not be completed until Hindu culture succeeds in reaffirming itself as universally Indian. The present struggle against the British rule is possibly the easier part. It is being conducted largely on the ground of sentiment, with a natural tendency toward chauvinism and prematurity, under the leadership of a great prophet, Mahatma Gandhi. But he cannot lead much longer, and if he could, it is highly questionable whether or not he could guide the deliberations of a legislative body. He overshadows so completely all the other figures on the Indian political stage that it is impossible to say to what degree competent men are available. There are a number of nationalists who have had experience in the legislative bodies, and may prove equal to the demands. India is no worse off than was Russia when the Bolshevik revolution was accomplished, and yet Russia has not failed. Next to Gandhi, the most powerful figure is probably that of Pandit Jawahar Lal Nehru, who is by many considered the hope of nationality. It would be idle to speculate. It is enough to say that the demand for leadership has not yet been sufficiently varied to reveal India's strength or weakness in that respect.

We hark back to the central theme of this brief discussion. No permanent peace is in sight for India until the rival cultures now present there have found a means of living together. Judging from the past history of India, we might expect to see the ultimate solution in an absorption by the indigenous Indic (Hindu) culture of the intruding foreign cultures, which are at present prospering but like the Hellenistic may finally have to give way. Plausible as this outcome may seem, it would be futile to make the prophecy. Another possible outcome would be for the general character of India's civilization to be altered in consequence of the

development of machine industry with the many changes it brings in life, the expansion of international communication, the diversion of public interest from religion to politics and sociology, and the influx of scientic knowledge from the West. That these last factors will produce profound changes in the civilization of India seems inevitable. Yet again the resulting national order may reasonably be expected to show characteristic Indic traits, modifying yet nevertheless continuing the same stream of culture that India has known for three—possibly five—millennia.

THE ADVANTAGES OF LESSENING RADIATION IN THE CYLINDERS OF INTERNAL COMBUSTION ENGINES

By FRANCIS I. DU PONT

(Read April 24, 1931)

IN THE short paper which I have the honor to present to this meeting, I would like to call your attention to some interesting scientific research which has resulted from a purely commercial need. A few years ago the General Motors research laboratory under Dr. Charles F. Kettering, assisted by Thomas Midgley, Jr. and T. A. Boyd, were engaged in an effort to perfect a unit for electric lighting on farms, etc. The particular effort was to make an engine operate with a fuel free from some of the fire risk of gasoline. In trying kerosene they encountered tremendous inefficiency in the particular engine they used.

A few experiments made in an effort to overcome this led them entirely away from their original purpose.

They discovered that there was quite a field for their effort in improvements to apply also to gasoline, producing a fuel which could be used in an automobile engine and which would lessen the knocking effect.

All efforts to overcome this up to that time had been directed toward mechanical improvements in the engine.

These men, however, held to the idea that the chemical conditions in the burning of the fuel had much to do with the subject. It was noticed that where knocking occurred this was accompanied by a marked reduction of thermodynamic efficiency. This could mean nothing but that less of the available heat was converted into mechanical energy and more was carried off by the water jacket around the engine cylinders.

In the investigation of this, the very ingenious expedient was resorted to of having a window in the engine cylinder made of fused silica. By the aid of this it was soon discovered

that with those fuels which gave most evidence of knock in the engine and showed a corresponding loss of thermodynamic efficiency a much greater intensity of ultra violet radiation existed. The first efforts to overcome this consisted in introducing the element iodine into the fuel, with the idea that the purple vapor of iodine would prevent to some extent this ultra violet radiation. This was found to produce a marked effect in the desired direction. Compounds of iodine were then tried and these also showed the same result. This was attributed to the decomposition by heat of the iodine compounds accompanied by the appearance of free iodine.

Next, smaller and smaller quantities of these compounds were tried and the effect continued till the amount was so small that it became doubtful whether the optical effect of iodine vapor was the cause.

Holding still to the idea of screening the radiation by colored substances some aniline dyes were tried and finally aniline itself.

This gave good results. Aniline while colorless to visible light is not colorless to ultra violet light as it has absorption bands in this part of the spectrum.

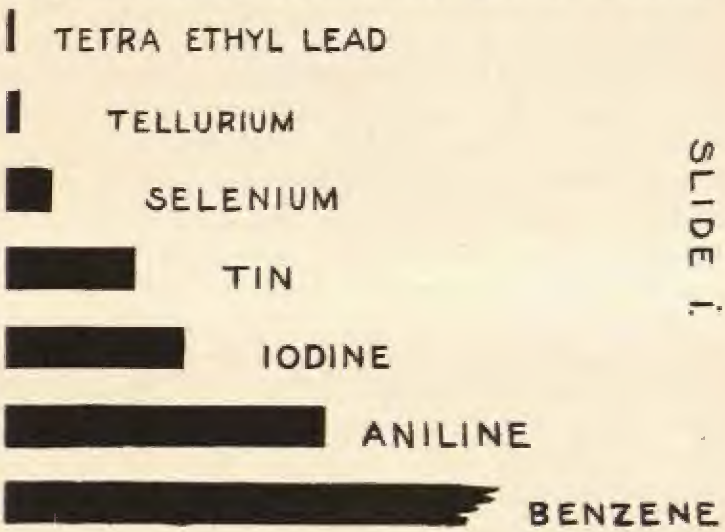
Other substances were tried and Tetra Ethyl Lead was found to be the most generally satisfactory.

The diagram (slide 1) shows the relative quantities of different substances required to produce a given beneficial effect. Perhaps benzene should not have been included in this comparison because all the others are used in quantities under 2 per cent while benzene must constitute at least one-half of the fuel in order to produce the result.

As the object of the study was to produce something really useful and for which there was a real need, the scientific study was naturally subordinated to the manufacturing problem. To be more correct, the scientific research was directed toward seeing if there was anything better or cheaper than tetra ethyl lead, and to the best method of manufacturing this. This very effort led to the scientific experiments which are the particular thing about which I wish to tell you. There was

arranged in connection with the engine cylinder a beautifully accurate indicator similar to a steam engine indicator, and with this the pressure at different points in the stroke of the piston was shown.

Instead of having a pencil arm of metal as has a steam engine indicator the indicator arm in this instrument was a beam of light thrown from a mirror arranged to tip by means of the pressure against a spring diaphragm in the wall of the



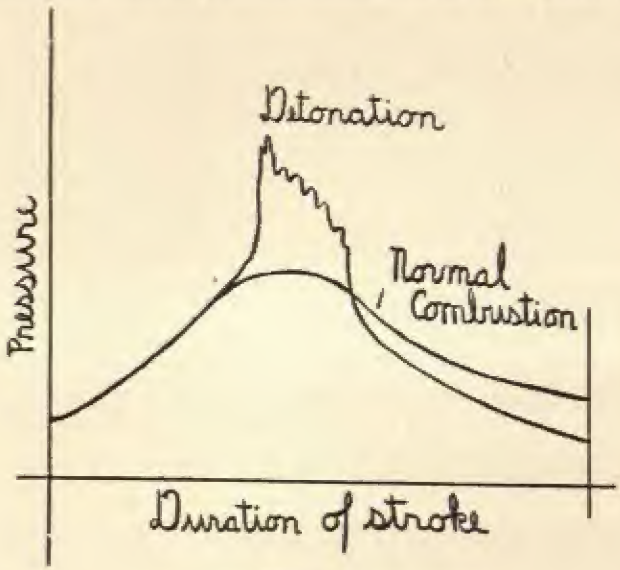
Slide 1.

engine cylinder. The diagram, of course, was traced on a photographic plate. This arrangement constituted a massless stylus.

At the same time the flame in the cylinder was photographed through a quartz window.

The indicator diagrams showed a sudden rise in pressure where a knock was noticed. This indicated a speed of increase in pressure corresponding to a detonation, and the photographs showed a tremendous amount of ultra violet light at this point, this being accompanied by a coincident loss of thermodynamic efficiency.

This can be illustrated by the curve shown: (slide 2). At the point where the curve becomes vertical, the rate of increase of pressure is of a different order of magnitude from the rate of speed of the engine piston, so that it is possible for but little of the heat generated during the particular part of the cycle to be converted into mechanical energy. The heat, therefore, must produce radiation which passes directly



SLIDE 2.

to the walls of the cylinder and is carried off as heat by the water jacket.

At first the wavy part of the curve after the point of maximum pressure was thought to be variations of actual pressure in the cylinder, but has been since found to be due to vibrations of the whole machine, due to the hammer like blow from the sudden increase in pressure.

The effect of the ultra violet light is probably twofold. Resulting from intensity of heat due to the fact that the relatively slow motion of the piston prevents, to a marked degree, the conversion into mechanical energy of the heat, this ultra violet radiation is probably a much quicker way to

convey energy to the water jacket than the conduction of heat by contact of the hot gases with the cylinder walls.

If we imagine the particles of the hot gases—molecules or perhaps atoms, for we do not know what happens to molecules at temperatures of intense incandescence—to radiate energy through the compressed mass of gases in the cylinder, as the stars radiate energy into space, we can form an idea of how too high a temperature might cause loss of energy to the water jacket in excess of what could be carried off by surface conduction.

When we look at the matter in this way, we must remember that the whole stroke of the piston of a high speed gas engine is accomplished in a very short interval of time and that that part of the stroke during which the intense radiation can occur is but a small fraction of this, so that the amount of heat which could be carried off by surface conduction would necessarily be very small. In surface conduction the relatively cool surface cools a layer of gas immediately in contact with it but can not cool the next layer till the cool layer has circulated out of the way. This takes time, and there is not time for a great amount of circulation during the stroke of a gas engine piston.

Radiation, however, can deliver energy through any number of layers, so to speak, of gas whether they be hot or cold.

The other probable effect of ultra violet light is that it activates the atoms of oxygen, hydrogen and carbon within their molecules, and thereby causes a quicker chemical action.

While this particular reaction proceeds at such a speed that a study of the rate of progress is nearly impossible, analogy with reactions whose progress is slow enough to study, would seem to adequately support this idea.

In addition to the intra-molecular effects of which the above explanation appears to be reasonable, there is good evidence from the pressure diagrams of a purely mechanical or physical effect. When the electric discharge ignites the compressed gases, combustion begins to spread throughout

the gaseous mass, and would be expected to progress according to some principle capable of algebraic expression if we could arrive at this. However, something else appears to take place which contributes to the undesirable effect. As the sphere of flame enlarges from the point of ignition, it compresses the mass of unignited gases ahead of it to the point where the heat, due to compression alone, produces ignition. We, therefore, have a secondary ignition which would at least double the rate of speed of the rise in pressure and thus account for some part of the sudden upturn of the curve in the diagram.

So much for our explanation of the undesirable effect.

The explanation of why certain materials introduced into the fuel in such minute quantities overcome the effect is not nearly as easy.

As little as 1/100 of 1 per cent of tetra ethyl lead when mixed with a gasoline which shows a tendency to knock, will affect this phenomenon very appreciably and increase to a very marked degree the thermodynamic efficiency.

The generally accepted explanation of those cases of chemical action where a very small quantity of some activating substance causes a great change in the chemical activity of large quantity of reacting substances, is what is known as a chain reaction. One molecule is conceived of as being activated by the catalyst, and this activates other molecules which in turn activate others.

In this way the catalyst could act upon relatively few molecules and yet on account of this multiplying principle, could produce a very extensive result.

With this hypothetical conception in mind, it is possible to form an idea of how a substance which has a tendency to interfere with this activating quality might stop the chain at or near its source, and we have an explanation, not altogether unacceptable, of why such small quantities of substances like tetra ethyl lead produce such a marked effect.

As this investigation progressed, the necessity for a better quartz window in the engine cylinder became apparent. For

mechanical reasons, all the original ones were circular. At the present time a long rectangular window is being used, and when it is remembered that this must stand an enormous pressure as well as great heat, it becomes evident that the construction of such an apparatus was quite a feat.

This window is arranged across the head of the cylinder, and the ignition is at one end of the same. The rate of progress of the flame can now be actually observed by means of cinematic photographs.

As this research is in progress at the present time the results are not quite ready for announcement. I may say, however, that the sensitized film moves simultaneously with the stroke of the piston and the flame front is clearly visible. The photographs show the sudden increase of rate of travel of the flame front, which would be expected from what the indicator diagrams show.

This work on the flame front is being conducted by Dr. Withrow and Mr. Boyd, and the spectrographic work by Prof. G. L. Clark.

It is interesting that a substance which seemed to be of interest only from a theoretical point of view should become of such importance industrially in such an unexpected way.

Tetra ethyl lead was first made in 1862. It was not investigated again till 1897 when some of its chemical and physical properties were determined. It was again made in 1916 but without any idea of practical utility. It is now manufactured in large quantities by the duPont Company for the Ethyl Gasoline Corporation, who are engaged in production of the various anti-knock gasolines, and who are conducting the interesting research about which I have spoken.

A few words about the manufacture of this material may be of interest.

In order that lead shall be in a condition to be soluble in gasoline, it is necessary to have an organic compound of this metal. Tetra ethyl lead affords us this condition.

It is well-known that lead compounds are all poisonous, but a lead compound soluble in oils and fats is far more danger-

ously poisonous than the ordinary lead compounds which are used, for example, in paints. Moreover, as this compound is volatile, poisoning occurs by inhalation of the vapors, and this poisoning is particularly insidious there being apparently no antidote or remedy after the vapors have been inhaled.

The manufacture of this material, therefore, presented, besides the usual problems of economy in manufacturing chemicals at a low cost, the problem of manufacturing this with safety, which was quite serious.

It has been solved by arranging the manufacturing process so that the materials are introduced into and the product taken out from a closed system. In the process ethyl bromide is made from ethyl alcohol and this is brought in contact with an alloy of sodium and lead in a finely divided state at a somewhat elevated temperature, and under pressure. In the reaction which occurs, the sodium takes the bromine from the ethyl bromide, and the lead being alloyed with sodium appears to become more active chemically than if this were not the case, so that the ethyl group joins to the lead atom without complications and the tetra ethyl lead is distilled from the mass left in the apparatus, the residue consisting in sodium bromide; a little sodium and a residue of metallic lead which is easily recovered and used over again.

The development of this into a safe, workable and economical process is the contribution made by the duPont Company to the establishment of a product which has become valuable to the producers of gasoline as well as to the consumers.

THE RETURN TO SHAKESPEAREAN ORTHODOXY

By FELIX E. SCHELLING

(*Read April 24, 1931*)

PERHAPS I ought to define this title before I begin, for orthodoxy is, of course, no very accurate term to employ as to the findings of science or even as to those of history. But there is, in the growth of opinion and scholarship on any topic, a middle way between timorous acceptance and eccentric theorizing that keeps steadily on, absorbing to itself after considered scrutiny such progress as can be made by the unearthing of new material or by a wiser understanding of what we have. It is in this sense that I employ the word "orthodoxy" in this paper, which has to do with the trend of critical opinion as to Shakespeare and his work, and the turn which criticism has taken of late towards an estimate less imaginative, less eccentric, if less certain of its findings, an estimate alike conservative, considered, and fair.

The variety of Shakespearean investigation is endless: there is obviously biography which is always with us, the archeology of the age, the topography of Elizabethan Stratford and London, stage history, so far as we can reconstruct it, the environment of Shakespeare in every use and misuse of that word, and, coming to the dramas themselves: there are endless bibliographical, textual, metrical and other questions, questions of the limits of authorship and collaboration, of authenticity, of origin and influences, even now of handwriting. Have we six or seven signatures of Shakespeare? on which nice matter depends his one time ownership of a copy of Montaigne's *Essays*, now in the British Museum, and much as to his reading by way of inference. Was he a well man or palsied when he signed his will? Have we or have we not a transcription in his own hand of the whole scene of a play on Sir Thomas More, the joint production of several playwrights,

according to a familiar Elizabethan custom? To the elucidation of this last an American scholar, Dr. Tannenbaum of New York, has brought to bear a whole new science—that of "bibliotics," as it was named by its inventor, the late Dr. Persifor Frazer, who, if I mistake not, was an honored member of this learned body.[1] And, lest I misrepresent Dr. Tannenbaum, whose brilliant scholarship deserves the highest admiration, let me say that as the verdict and conclusion of a long and interesting debate involving many scholars, their books and pamphlets, we must now reluctantly accept the decision that this attempt to extend the canon of Shakespeare to include a scene written by the dramatist's own hand in a play called *Sir Thomas More*, has not made good its case.

In a paper of this brief scope I cannot attempt even a bare recital of the recent activities of Shakespeare scholarship and criticism. Let us take a few points at random, with at least an allusion to the inferences involved. A damaging commonplace of the popular myth as to Shakespeare assumes that he was peasant-born, in a backward, inland village; that his father was illiterate and towards the end of his life a bankrupt; that the boy could have had no education and that it is incredible that out of such conditions such results as the plays should have come. We must now correct all this. Stratford was a thrifty market town, supporting a Latin master and a parson, both of them University bred men. The father of Shakespeare was not only entirely literate, but he rated among gentlemen and freeholders. His retirement towards the end of his life was neither due to bankruptcy nor to his recusancy as one of the Roman faith; but rather to a leaning towards liberal if not even Puritan opinions.[2] Here is a new paternity for Shakespeare, to which we may add an anecdote, culled from an out of the way diary, in which Shakespeare's father is described as a "merry cheeked old man that said 'Will is a

[1] S. A. Tannenbaum, *Problems in Shakspere's Penmanship*, 1927; and *The Books of Sir Thomas Moore, a Bibliotic Study*, 1927. See also A. W. Pollard and Others, *Shakespeare's Hand in the Play of Sir Thomas More*, 1923.

[2] E. I. Fripp, *Shakespeare Studies*, 1930; and his *Master Richard Quyney*, 1924, Introduction, and p. 78.

good, honest fellow, but he durst crack a jest with him at any time.'" As to Shakespeare's own alleged illiteracy, a disquality which can scarcely be considered congenital, a nicely written little letter in Latin, the penning of a boy, Richard Quiney, of Shakespeare's years and his equal precisely in station, now turns up, written from school to his father, a Stratford merchant, who could evidently read it. Again, an exhaustive examination into the question as to whence Shakespeare derived his wealth of classical allusions discloses that the commonly accepted notion that he borrowed it out of the whole cloth from Golding's clumsy translation of Ovid's *Metamorphosis* into English is quite untenable. Word, phrase, manner and sequence of thought declare Shakespeare's close and loving familiarity with the Latin poet in the original, a popular school book of the day, which the poet evidently must have studied when he attended Stratford Grammar School.[1] Apropos of all these allegations of a lack of education on the part of our great dramatic poet, it has been declared that our popular denial that Shakespeare was ever a student at Oxford or Cambridge is based merely on the Registers of Matriculation and Graduation at either university. Oliver Cromwell neither matriculated nor graduated and hence is unnamed and unknown at Cambridge, where he resided for a year a student of Sidney Sussex College. Now there are thirty-one colleges at Oxford and twenty at Cambridge. There were not so many in Shakespeare's day, but Dr. Smart none the less pertinently concludes: "Until the archives of the Colleges for the appropriate dates have been minutely searched, and no William Shakespeare discovered, it will be impossible to prove from University sources that he was never at Oxford even for a term."[2]

But let us turn from this matter of Shakespeare's alleged lack of learning, to his as strenuously alleged lack of morals. (It never seems to have occurred to anyone that essentially bad men seldom write elevated literature.) These allegations

[1] *Ibid.*, "Shakespeare's Use of Ovid's *Metamorphosis*," p. 98 ff.

[2] J. S. Smart, *Shakespeare, Truth and Tradition*, 1928, p. 176.

appear to be based on the circumstance that Shakespeare married—well, perhaps it is best expressed by prematurely, if not precipitately. Anne Hathaway might have been in a worse plight if Will had not married her: he did not desert her. A second allegation is Shakespeare's neglect of Stratford: this turns out to be a negative matter, remaining to be proved. How does anybody know how seldom or how often he returned to Stratford? [1] to which we *do* know he hastened permanently to return as soon as his pressing business with the stage allowed. Of all the stories invented about Shakespeare, none is as engaging and intriguing as that of the dark lady. Books have been written on it, plays have been staged about it. In modern biography, as in crime—see Strachey and especially Maurois—*toujours cherchez la femme*. And generations ago the dark lady of the *Sonnets* of Shakespeare was mysteriously discovered, to produce before long a whole library of conjecture as to who this discreditable personage might really have been.[2] There are quite a number of things that we do not know about Shakespeare *Sonnets*, things that it is quite likely that we shall never know. But why, in an age notorious for its sheer imaginativeness, in which sonnets were constantly written, often avowedly to nobody at all—why in the case of a dramatist whose very art demands that he be objective and take you in with the reality of a Lear, a Macbeth and a Hamlet who never existed—why, I say, in the name of common sense, are we constrained to consider these *Sonnets* of Shakespeare as so certainly autobiographical? However, now for a refreshing example of the stubbornness of facts. The conditions of the biographical dark lady are first obviously that she be dark—one excellent, if unimaginative, German investigator of dark ladies argued for a mulatto; a second condition demanded that she be a "lady," who, however doubtful her morals, was at least the infatuated poet's

[1] Aubrey says that Shakespeare was wont to go to his native country once every year.

[2] A. Acheson, *Mistress Davenant and the Dark Lady of the Sonnets*, 1913; Countess de Chambrun, *The Sonnets of Shakespeare*, same date; and see P. Butler, *Materials for the Life of Shakespeare*, 1930, p. 45 ff.

social superior. The two most conspicuous claimants for the distinction of having successfully "vamped" the world's greatest dramatist are a certain "lady" of the court, Mistress Mary Fitton, who turns out on the discovery, some time since now, of two authentic portraits of her, to have been a pronounced blond;[1] and the second claimant, the wife of an excellent innkeeper at Oxford, to whose son, William Davenant, Shakespeare stood as godfather at his christening, while unquestionably a brunette and a very charming woman, was not a "lady" in the Elizabethan conception of that word, but was celebrated in contemporary verses, once preserved at Warwick Castle, as a virtuous and exemplary wife.[2] I should be genuinely sorry to see the anonymity of this great shade, this shadow of a poet's imagination, as wicked and beautiful as Lilith and as real, haled into the sunlight, her heartstrings dissected at some psychologists' clinic, and the wraith, the mystery, the precious unreality of it all reduced to the certainty and the curiosity of a detective story.

The "biography" of Shakespeare, to leave it for more important things, has been written, rewritten and miswritten to a frazzle. Not a guess has been unguessed or a distortion undistorted. And yet, to quote the words of the very latest writer upon him, "Shakespeare, the man, is transparent and inscrutable"; in a word, the outward life of Shakespeare is quite the least important thing about him.[3] For, unlike many lesser men who can hold up the mirror of their art only before their own petty personalities, here was a master who achieved that detachment, that oblivion of self in which alone can any art hope to reach a universal significance.

Within the last twelvemonth a monumental *Study of the Facts and Problems* which attach to the name and works of Shakespeare has appeared, by the notable English scholar, now, for his services to scholarship, Sir Edmund Chambers.[4] Chambers began this work of his many years ago, with a

[1] Lady Newdigate, *Gossip from a Muniment Room*, 1897.
[2] J. O. Halliwell-Phillipps, *Outlines of the Life of Shakespeare*, ed. 1887, ii. 48.
[3] J. W. Makail, *The Approach to Shakespeare*, 1931.
[4] E. K. Chambers, *Shakespeare, a Study of the Facts and Problems*, 1930, 2 vols.

careful and scholarly appraisal of the mediæval conditions that led in time to the amazing outburst of dramatic aptitude and artistry which we call Elizabethan Drama. He then proceeded to a study of that drama in its minutest details, sifting and adjudging the many, often divergent, views that had obtained among his predecessors; and thus equipped, with two splendid works to his credit, he undertook this study of Shakespeare, the central figure, in a full realization of his relations to his time. It may not be remembered by all that the text of Shakespeare's plays as we read it to-day in modern editions is the result of the study, the correction, the emendation of some seven or eight generations of scholars; for while, contrary to popular opinion, most of the early editions of these plays, individually or collectively, were not really very bad according to the somewhat careless methods of the time, it is equally true that there are many places in which there is need, not only of specific knowledge, but of the application of that species of genius that goes alike to the attainment of truth whether it be in the discovery of a new chemical property or in the restitution to integrity of a poetical figure overlaid by a misprint. But the activity of scholars, here as elsewhere, has not stopped at a restitution of the text. It has gone further, as in the Homeric question or in matters appertaining to the text of the Bible itself, to doubt, to question and to disintegrate, sometimes with adequate and interesting results, at times with a stubborn skepticism that seemed begotten far more of egotism and a desire for the destruction of singularity rather than for the scholar's love of truth. It is impossible here to do more than suggest the nature of some of the many problems of text and authorship with which this encyclopedic work is concerned. For example, there is a no more vexed or difficult question than that which concerns the history of the stage in the age of Shakespeare: the facts by way of record are scattered and incomplete; the companies were many and confused, alike in their patrons and their personnel, both of which were open to incessant change, shifting, coalition, division, appearance now at Court, again in London, and

fitfully in the provinces.[1] Contrary to the neat theories of Sidney Lee and others that trace Shakespeare as a boy attracted by the Earl of Leicester's theatrical entertainments at Kenilworth into the orbit of the players, Sir Edmund finds Shakespeare's emergence as a player and playwright in the theatrical disorganization of the years 1592 to 1594, circumspectly allowing the alternatives of an association with Pembroke's men, with Sussex's, or even the status of an unattached playwright.[2] And he rejects another recent surmise which reconstructs for Shakespeare a regular period of seven years' apprenticeship, and ties him to a system in which his art was largely determined by the personalities and the limitations of his players.[3] On the more solid ground which follows, our knowledge of Shakespeare as "a payee on behalf of the Chamberlain's men for plays given at court in the winter of 1594," we progress forward rapidly in the familiar story of the dramatist's career, coming again and again upon adjudications or *obiter dicta* which illuminate the theses or refute the arguments of less governed scholarship.

One of our prevalent departures into the realms of allusive fancy, where historical analogies exist in the atmosphere of a world of four dimensions, transforms Shakespeare's dramatic transcripts of history and story into deeply planned allegories of contemporary or recent Elizabethan events. According to this species of twist as to the significance of things, Hamlet becomes an adumbration of King James before his accession to his English throne, and the story of his mother, Mary Queen of Scots, and Bothwell is set forth in that of Macbeth and his lady.[4] Here is our author's wholesome verdict as to that sort of thing: "I do not myself believe that, apart from some

[1] E. K. Chambers, *The Elizabethan Stage*, 1923, 4 vols., especially ii, 1–295.

[2] S. Lee, *Life of Shakespeare*, ed. 1925; and Chambers, *Shakespeare*, as above, i, 56–94.

[3] T. W. Baldwin, *The Organization and Personnel of the Shakespearean Company*, 1927.

[4] See L. Winstanley, *Hamlet and the Scottish Succession*, 1921; *Macbeth, King Lear and Contemporary History*, 1922; E. Rickert, "Political Propaganda . . . in the Essex Conspiracy," *Modern Philology*, 1923; or E. M. Albright, "Shakespeare and the Essex Conspiracy," *Publications of the Modern Language Association*, 1927.

passages of obvious satire in comic scenes, there is much of the topical in Shakespeare, whose mind normally moved upon quite another plane of relation to life."

An absorbing and informing chapter of this work is that entitled "The Book of the Play," the phrase employed contemporaneously to denote not the author's manuscript or "original," but that manuscript as ordered, corrected, and otherwise arranged to guide the prompter or bookkeeper in his direction of the play. Scholarship of late has been very active in this matter of Elizabethan dramatic manuscripts, of which an interesting variety—though, alas, none certainly of Shakespeare's—is extant; and the work of Greg, Sisson, Sir Edmund himself, and many others has made many of these accessible and studied them with illuminating results to our knowledge of things, extending from the handwriting of the period, as suggested above, to the possible effects of manuscript peculiarities and imperfections on the text of plays subsequently in print.[1] In its wider reaches this is a subject of great intricacy, involving, as it does with much else, that maze of difficulty, the Elizabethan practice of collaboration in playwriting. Of late, a theory has been evolved, especially in the hands of Dr. J. Dover Wilson, one of the editors of the New Cambridge Shakespeare, by which—to express the idea somewhat by way of exaggeration—a species of "continuous copy" is surmised as having existed in the possession of the theatrical company, begun originally by one or more writers and open to a succession of rewritings and revisions subsequently at the hands of apparently anybody who might be employed for the purpose.[2] Such a theory, in the upshot, transforms Shakespeare, with most of his fellows, into a mere "play-patcher," mending and amending, without original or artistic purpose or design; and arrogates, on the part of a critic such as Mr. J.

[1] Chambers gives a list of extant Manuscript Plays, running to upwards of fifty. *Elizabethan Stage*, iv, 404.

[2] See especially Dr. Wilson's "Introduction" to *The Tempest*, xxxiii, 79, and Chambers, *Shakespeare*, i, 152. As an example, J. M. Robertson, "The Authorship of The Comedy of Errors," *The Shakespeare Canon*, 1925, ii, 126; and A. Gaw, "The Evaluation of the same," *Publications of the Modern Language Association*, 1926.

M. Robertson, a preternatural discernment in the niceties of vocabulary, diction, and the use of figure by which these writers of three hundred years since are delicately to be distinguished. Sir Edmund, as might have been expected from his brilliant lecture on "The Disintegration of Shakespeare," a few years since, will have none of this; and a general vindication of the Shakespearean text from every point of view is not the least valuable of the many services of his scholarship and that, too, especially of A. W. Pollard, who has written so informingly on the Folios and Quartos of Shakespeare.[1]

In these hasty words I have only scratched the surface of a topic that must always take first place in the consideration of literary genius. Possibly some of us may console ourselves that we can read our Shakespeare and measurably understand him, innocent of all this coil of scholarship and ignorance about him.

[1] *Proceedings of the British Academy*, 1924; A. W. Pollard, *Shakespeare Folios and Quartos*, 1909.

OSMOTIC HEMOLYSIS AND ZOÖLOGICAL CLASSIFICATION [1]

By M. H. JACOBS

(*Read April 25, 1931*)

The red blood corpuscles of the different species of mammals are so similar in their structure and in their general appearance as to be almost indistinguishable from one another. With the exception of the elliptical shape of the corpuscles of the camel and its relatives, the only known differences are in size, and these are frequently too slight to be of any practical value for purposes of identification. In the case of the nucleated erythrocytes of the lower vertebrates, the differences may at times be somewhat greater, but they are usually far from striking, and even here the cells of a given species or group of related species can rarely be recognized with certainty by morphological characters alone.

It is of interest, therefore, to find that certain species as well as certain larger groups are characterized by physiological peculiarities which are both constant and easily recognized. By means of such characters not only may individual species be identified but frequently unmistakable evidences of zoölogical relationship may be traced throughout a group of similar forms.

There will here be discussed briefly certain characteristic differences in the rates of osmotic hemolysis in approximately 50 species of vertebrates which have been studied during the past three years with the assistance of Messrs. W. A. Smith, A. K. Parpart and G. E. Shattuck, whose valuable aid in making the numerous and time-consuming observations is hereby gratefully acknowledged. The method employed

[1] A part of the work here reported, which is still in progress, was made possible by financial aid from the Board of Graduate Education and Research of the University of Pennsylvania.

was that recently described by the author.[1] The species studied included 10 mammals, 35 fishes, 2 amphibia, 3 reptiles and 3 birds. In the case of most of these forms, a sufficient number of individuals were examined to give grounds for considerable confidence in the constancy of the results reported. In several other cases, however, the number of individuals has as yet been so small that the results must be considered as preliminary rather than final.

If a red blood cell be placed in water or in a strongly hypotonic salt solution (*e. g.*, 0.02 M NaCl), osmotic forces cause it to swell until it gives up its hæmoglobin to the surrounding solution and becomes invisible. The time required for hemolysis in water of cells of approximately average osmotic resistance ranges from less than ½ second in the sheep to a minute or more in some of the sharks. This time depends not merely on the permeability of the cell to water but on a number of other factors as well, among them being the critical hemolytic volume of the cell, the ratio of its surface to its volume, the resistance to stretching of its membrane, etc.

In Table 1 are shown the times in seconds required for a definite degree of hemolysis (75 per cent) to be attained when the red blood cells of various vertebrates are suddenly exposed to water. It will be noted that evidences of zoölogical relationship appear unmistakably in these figures. Thus, the six species of the Elasmobranch fishes studied are sharply separated from upwards of 30 species of Teleosts, of which 13 have here been separately listed. In but two of the cases reported does the time of hemolysis for a Teleost exceed 2.5 seconds, while the figures for the Elasmobranchs range from 12.5 to 65.5 seconds. The only Amphibia so far studied appear to resemble the Elasmobranchs rather than the Teleosts, though the data for this group are as yet very incomplete. In the reptiles, represented by three species of turtles, the figures are much lower; while those for the mammals are lowest of all, ranging from 2.4 seconds in man to a fraction of a second in the sheep. As yet, only three birds

[1] Jacobs, M. H. *Biol. Bull.*, 58, 104–122, 1930.

TABLE 1

TIME FOR 75 PER CENT HEMOLYSIS IN WATER AT 20° C.

	No. of individuals	Time in seconds
1. Fishes		
(a) Elasmobranchs		
Sting ray	1	13.0
Winter skate	1	12.5
Spring dog-fish	1	14.8
Tiger shark	1	65.5
Sand shark	1	42.5
Dusky shark	1	49.5
(b) Teleosts		
Mackerel	9	2.0
Chub mackerel	4	2.4
Bonito	5	2.3
Alewife	17	1.7
Menhaden	4	1.5
Hickory shad	6	1.5
Tautog	4	6.6
Cunner	3	1.9
Carolina sea robin	2	2.1
Striped sea robin	3	2.1
Butterfish	2	1.2
Bluefish	5	1.8
Goosefish	1	7.5
13 other species (average)		2.2
2. Amphibia		
Frog (Rana clamitans)	4	71
Frog (Rana pipiens)	1	57
3. Reptiles		
Snapping turtle	2	6.1
Eastern painted turtle	2	5.3
Loggerhead turtle	1	3.2
4. Birds		
Chicken	3	26.3
Pigeon	2	1.6
Duck	2	1.7
5. Mammals		
Man	9	2.4
Dog	7	1.8
Guinea-pig	7	1.5
Rat	8	1.4
Ox	8	1.3
Rabbit	3	1.1
Cat	4	ca. 1.0
Pig	4	ca. 1.0
Mouse	5	< 1.0
Sheep	3	< 1.0

have been studied and it will be noted that within this group the figures, as far as they go, show far greater differences than is the case, for example, with either of the two sub-groups of the fishes or with the mammals.

Within a single zoölogical group, differences in the times of hemolysis in water may frequently be used to separate related forms. Thus the erythrocytes of the sheep may readily be distinguished from those of the ox by their much more rapid rate of hemolysis, the rate for the sheep being, in fact, too rapid to be measured by the method here employed, while that for the ox can always be determined with considerable accuracy. Another striking example of such differences, based on a smaller number of observations but apparently constant, is that between the tautog and the nearly related cunner. Most remarkable of all is the difference between the chicken, on the one hand, and the duck and the pigeon on the other.

In the type of hemolysis so far dealt with, water alone penetrates the cells. A more complicated type is that in which the entrance of water is dependent upon the simultaneous penetration of some dissolved substances. In this case the permeability of the cell to the substance in question is an important factor in determining the time of hemolysis. It would be expected that if characteristic differences between different species and groups of species are found in the simpler case where water alone is concerned, such differences would become proportionately more striking when permeability to an additional substance is involved. This proves to be true.

It is not always easy to separate the rate of penetration of water from that of the dissolved substance, but a rather rough measure of the latter may be obtained by subtracting the time of hemolysis in water alone from that in a solution of the penetrating substance isosmotic with the blood and dividing by the time of hemolysis in the solvent alone. The resulting figure gives the retardation due to the dissolved substance as a multiple or a fraction of the time otherwise required. This method of comparison, while not theoretically perfect, is

convenient and perhaps sufficiently accurate for present purposes and has been used for the calculation of the figures listed in Table 2. In this table the letters S, U, E and G refer to the times for hemolysis in the solvent alone and in solutions of urea, ethylene glycol and glycerol, respectively, of concentrations approximately isosmotic with the blood of the species studied. Partly because these concentrations vary somewhat from class to class and partly because the solvent used was 0.02 M NaCl in the case of the birds and the mammals and water alone in the case of the other groups, the figures in the first three columns are not strictly comparable. Those in the last two columns are much more so, however, since they merely give a comparison between the penetrating powers of urea and ethylene glycol and of urea and glycerol under otherwise identical conditions. The presence of low concentrations of NaCl has been found in the case of the fishes to change the ratios in question slightly, but not sufficiently to cause any very significant differences from the results here reported.

It will be noted on examining column 4 of Table 2 that the relative penetrating powers of urea and ethylene glycol show characteristic differences from group to group. Permeability to urea is relatively much greater than that to ethylene glycol in all of the mammals so far studied, while the reverse is true in all of the fishes. As far as the rather scanty data for the other groups permit conclusions to be drawn, the turtles approach the mammals with respect to this character, while the Amphibia and birds resemble the fishes.

Of even greater interest is the comparison given in column 5 between the behavior of urea and glycerol. These substances are known to penetrate most cells at rates which are at least of the same order of magnitude. In the case of mammalian erythrocytes, however, and to a somewhat lesser extent those of the turtles and of the frog, the rate for urea is incomparably greater than that for glycerol, while the erythrocytes of the fishes have retained the apparently primitive condition found in ordinary cells. It is of some interest to

TABLE 2

Species	(U-S)/S	(E-S)/S	(G-S)/S	(U-S)/(E-S)	(U-S)/(G-S)
Spiny dogfish	35.4	0.7	181	51.0	0.20
Sting ray	24.0	0.6	27	37.8	0.90
Sand shark	35.8	1.2	181	28.1	0.19
Mackerel	43.5	0.9	20	51.2	2.22
Chub mackerel	39.0	0.7	16	55.0	2.42
Bonito	66.8	1.0	8	67.0	8.71
Alewife	2.3	1.6	40	2.7	0.11
Menhaden	2.3	2.1	18	1.1	0.13
Hickory shad	27.0	2.5	59	10.9	0.45
Tautog	45.0	0.9	74	47.8	0.61
Cunner	87.4	1.4	115	61.6	0.76
Carolina sea robin	245	3.0	261	79.2	0.94
Striped sea robin	266	3.0	342	88.5	0.77
Frog	0.2	0.2	19	1.2	0.011
Snapping turtle	0.4	4.4	570	0.08	0.0006
Eastern painted turtle	1.6	3.3	458	0.49	0.0036
Loggerhead turtle	1.6	21.6	2150	0.07	0.0007
Chicken	18.6	0.5	73	25.4	0.19
Pigeon	24.8	1.0	1	24.1	26.7
Duck	82.2	1.7	2	48.7	33.5
Man	0.07	0.5	4	0.143	0.0173
Mouse	0.20	1.9	12	0.107	0.0167
Rat	0.14	0.6	4	0.250	0.0406
Rabbit	0.17	2.2	22	0.077	0.0078
Guinea-pig	0.12	2.1	38	0.056	0.0031
Cat	0.22	5.8	450	0.038	0.0005
Dog	0.10	3.7	252	0.027	0.0004
Pig	0.20	4.6	340	0.044	0.0006
Sheep	0.31	11.7	850	0.027	0.0004
Ox	0.16	8.2	610	0.019	0.0003

find that the sharpest break with respect to this character is not between the forms possessing nucleated and those possessing non-nucleated erythrocytes but rather within the latter group.

In addition to these more general characteristics of the larger groups, a number of examples are found in the case of smaller zoölogical groups. For example, the three representatives of the mackerel family, which are grouped together in the table, are all characterized by an extremely high (U-S)/(G-S) ratio, while the same ratio for the three species of the herring family which have so far been studied, namely, the alewife, the menhaden and the hickory shad, is in each case unusually low. It is very unlikely that these results are to be explained as being due merely to coincidence.

Further examples of family resemblances are to be found in the figures for glycerol in column 3 where in the case of a single group such as the mammals all of the figures are comparable. It will be noted that the four rodents studied (the mouse, rat, rabbit and guinea-pig) all show low values for the delay in hemolysis due to glycerol, while at the other extreme we have very high values for the pig and especially for the sheep and the ox. Similarly, among the fishes the low values for the mackerel and the herring families may be contrasted with the high ones for the two species of sea robins. The most striking difference of all is that found in the birds between the chicken on the one hand and the pigeon and the duck on the other. Particularly by the use of glycerol is it possible in this way to distinguish various species and groups of species from others which fall within the same larger class.

The results here described are as yet very incomplete. As far as they go, however, they seem to demonstrate that the erythrocytes of various groups of vertebrates, as well as those of individual species, may possess striking physiological properties which are frequently more prominent than their morphological ones and which, to a considerable extent, seem to parallel the zoölogical classification of the forms in question. It is believed that a further study of such peculiarities may

be of interest not merely in making visible specific and other differences which would perhaps otherwise forever remain invisible, but in furnishing to the cell physiologist the useful tool of comparative physiological knowledge for attacking such general problems as, for example, that of cell permeability.

PRELIMINARY RESULTS ON THE APPLICATION OF THE MOTION PICTURE CAMERA TO CELESTIAL PHOTOGRAPHY

By FRANCIS C. MCMATH, HENRY S. HULBERT, ROBERT R. MCMATH

(*Read April 25, 1931*)

THE moving pictures of the Moon and Jupiter which were presented before the American Philosophical Society April 25, 1931, by Dr. Heber D. Curtis, Director of the Detroit Observatory of the University of Michigan, Ann Arbor, were taken at the McMath-Hulbert Observatory located at Lake Angelus Village, Oakland County, Michigan.

The telescope at this Observatory is a $10\frac{1}{2}$ inch Pyrex Cassegrain Reflector with a Bruce type mounting, focal length 180 inches. The guide telescope is a four-inch Bausch and Lomb refractor of sixty inch focal length.

The camera is a standard Eastman Cine Kodak using their 16 mm. film—its mounting is quite clearly shown in the photograph—Figure 1. The photograph also shows the focal tester, which is used to make sure that the picture is properly centered on the film, and the flexible shaft and gearing used to operate the camera. The eye end of the guide telescope is also shown.

The telescope is driven in right ascension by Telechron motors using sixty cycle current from the Detroit Edison Company who regulate their cycles very accurately to standard time. By introducing gears with a ratio of 365 to 366, a very close approximate to the sidereal rate is obtained. This driving clock is described in detail in a paper by Robert R. McMath, published in the October, 1930, number of Popular Astronomy. The operation of this drive has proven very satisfactory.

The work of taking motion pictures is very exacting as it is necessary to guide very accurately over periods of time,

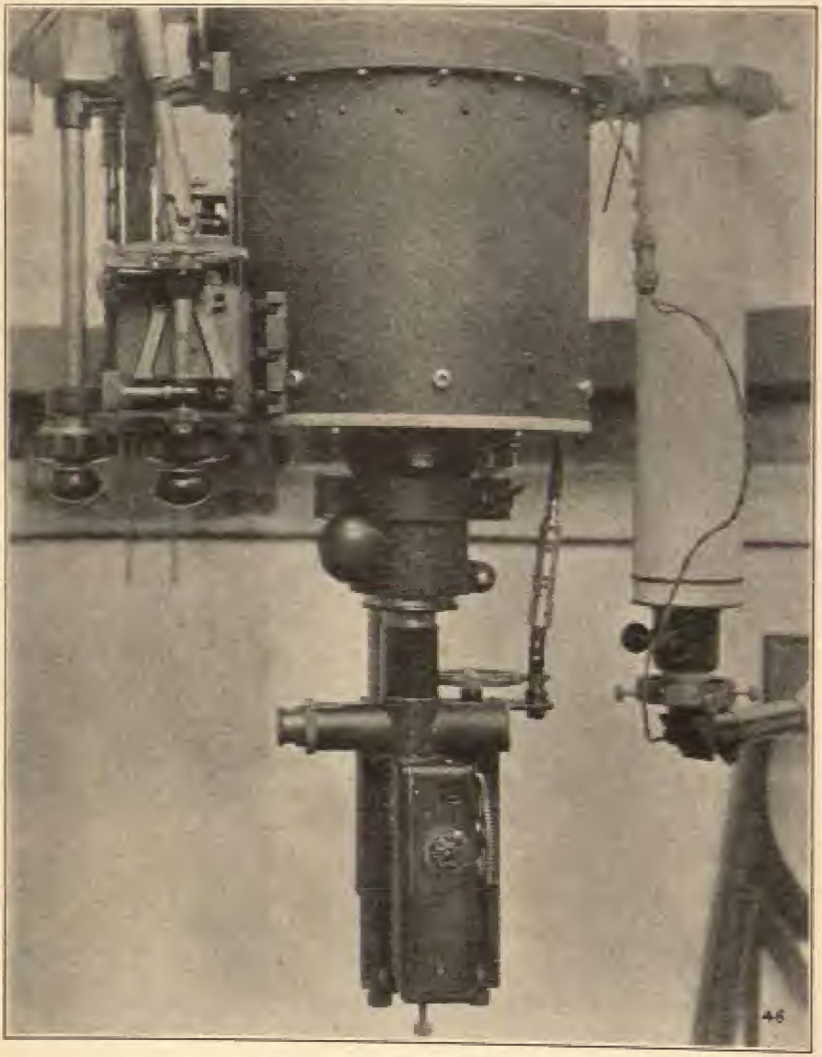


FIG. 1.—Eye End of Telescope.

usually from four to eight hours. If pictures are being taken at the rate of four per minute, there will be 240 separate photographs taken each hour. At the end of five hours there will be 1200 astronomical photographs, which must be as nearly exact as possible in registration. It is for this reason that the work of guiding is continuous and extreme accuracy is essential. To reduce the effort of the operator, the slow motions in both right ascension and declination are operated by small electric motors by means of push buttons conveniently arranged for operation from the observer's chair.

When the Moon is the subject, the guiding problem is very difficult because of its rapid motion in declination and also because of its changing parallax. The change in declination over the lunar month may range from zero to eighteen seconds of arc per minute of time in either direction. The apparent motion of the Moon in declination from changing parallax is a serious matter also, as it may range during an observation from zero to about five seconds of arc per minute of time.

It should be noted that the apparent motion of the Moon in right ascension is also affected by changing parallax and refraction. The conventional lunar gear is designed to drive the telescope at an approximation of the geocentric rate. For long runs while making motion pictures, it is very desirable to have a method of changing the clock rate. To secure this result we have designed and are building a special frequency changer, which will be introduced between the electric power supply and the telechron motors which are driving the telescope. This frequency changer will make lunar guiding in right ascension as flexible and convenient as that which has been secured in declination.

To simplify guiding on the Moon, a table has been made with the assistance of Doctor Maxwell of the Detroit Observatory, showing the composite effect of declination, parallax and refraction changes with the hour angle, so that on any date and hour desired, the composite rate of change in declination to be expected can be taken from the table. Usually this composite rate differs quite materially from the

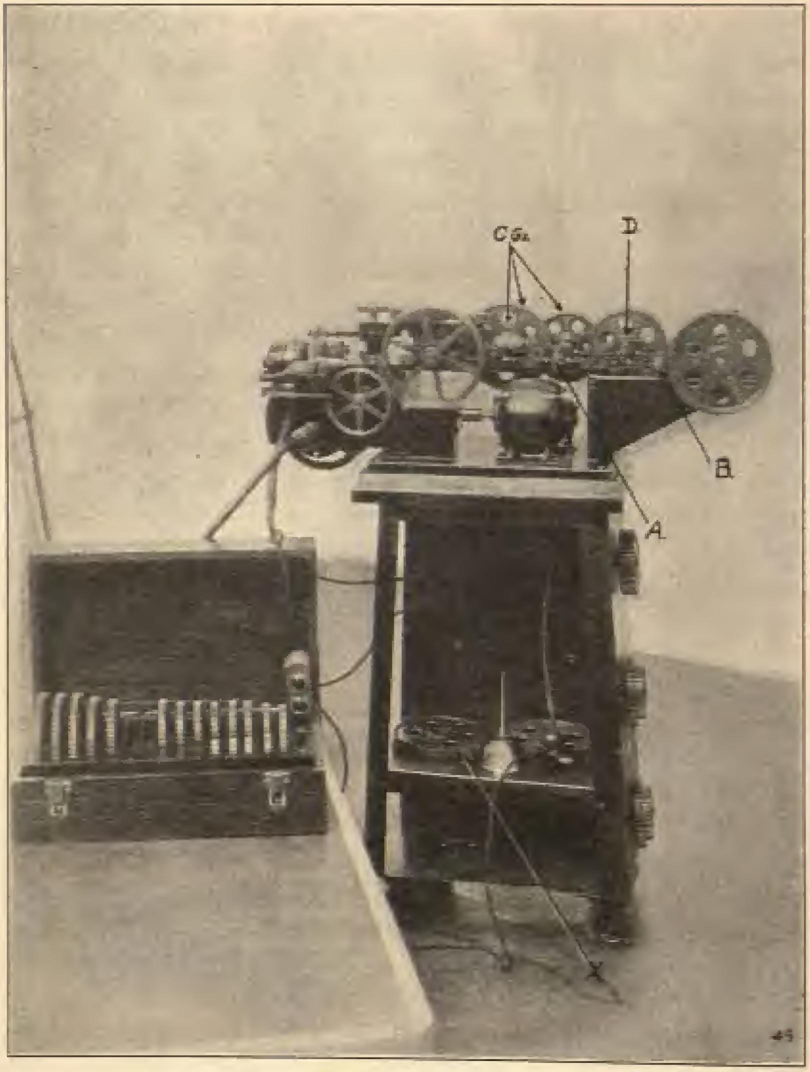


FIG. 2.—Camera and Lunar Declination Drive.

geocentric rate of change in declination given by the Nautical Almanac and Ephemeris.

In a first attempt to provide a large range of changes, a friction disc was introduced in the declination slow motion, but it proved unsatisfactory and it was necessary to design the gearing shown in Figures 2 and 3.

Figure 2 is a photograph of the combined camera and lunar declination drives. The camera drive which will be referred to later is on the right hand bracket of the base. The motor in the center runs at half synchronous speed, which through a worm and gear reduction, provides a constant shaft speed of 18 R.P.M. Power for both drives is taken from this shaft.

Figure 3 is a large scale picture of the declination drive. The synchronous motor with its worm and gear, is here shown in the lower right hand half of the picture. The gear *D* is driven by the constant speed shaft through a pair of spiral gears and reversing gears. These latter are controlled by lever *R*. Reversing gears are necessary because the Moon's motion in declination may be either North or South. Gears *A*, *B*, *C* and *D* constitute a compound train. By changing the gears in this train a large range of speed is obtained. A total of twenty-four gears is necessary to give a range from one to twenty seconds of arc per minute of time in steps of approximately one third second of arc per minute. This range completely covers the rates necessary for lunar photography. Gear *A* drives flexible shaft *S* through a planetary train (not visible in picture). This flexible shaft drives the declination slow motion at the eye end of the telescope. This slow motion is clearly shown in Figure 1.

Although the apparatus described above, using the proper gears, gives a very close approximation to the moon's rate of change, compensatory guiding is still necessary. This is accomplished by the device shown on the left of Figure 3. It consists of Motor *M*, a series wound reversing motor driving worm gear *W*, which turns a pinion engaging the ring gear of the planetary train. A magnetic brake, *T*, is placed on the motor shaft to prevent over run, thus giving more delicate

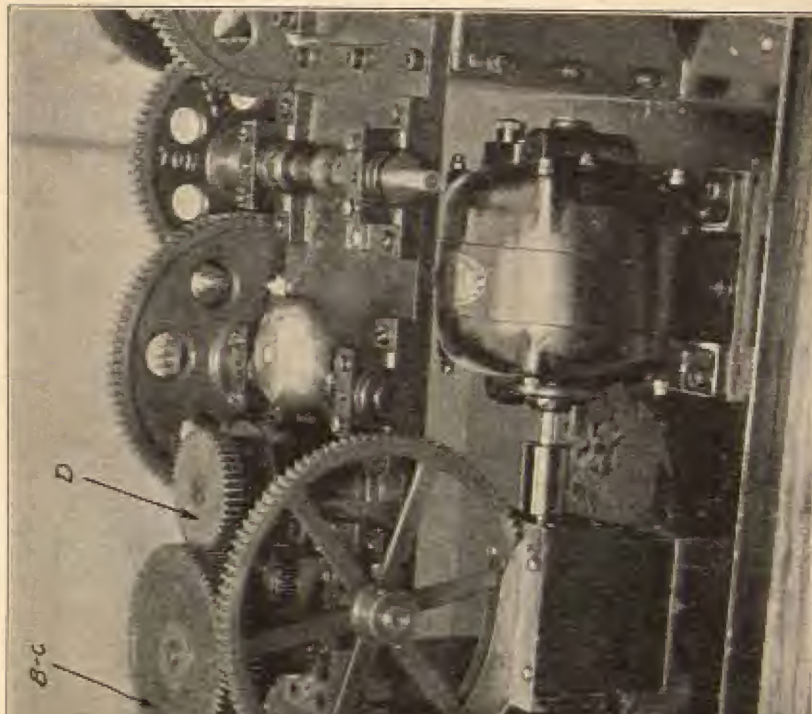


FIG. 3.—Lunar Declination Drive.

control. This device provides a slow motion in either direction, superimposed on the declination drive. Motor *M* is controlled by push buttons convenient to the observer's chair. In use changes in the declination rate can be made in a few seconds. The entire apparatus has proven very satisfactory and is believed to be novel.

A fundamental difficulty in celestial motion picture photography is to secure a satisfactory relation between length of exposure and the number of feet of film necessary to give adequate projection time on the screen. It is therefore essential in taking pictures, to balance the time of light and dark on the film so that on any predetermined length of run, the movement of the celestial subject will be satisfactorily shown on the screen by a length of film which is neither too short or too long. For example, if a five hour run is decided upon for Jupiter's satellites, a good exposure time would be seventeen and one half seconds and the dark time two and one half seconds, which requires four and one half feet of 16 mm. film per hour, making a total length of film of twenty-two and one half feet, resulting in a projection time on the screen of nearly one minute—which is satisfactory. For the lunar terminator using primary focus, a good timing is one and one half seconds exposure and thirteen and one half seconds dark. Using this set-up a good shadow change can be shown in a four hour run. When desired the camera shutter may be turned at constant speed in the usual manner giving the ordinary equal intervals of light and dark.

The camera drive device is essentially as follows: In Figure 2, stub shaft *A* is the point of attachment for the flexible shaft when using the constant speed type of drive just referred to above. By means of compound change gears *C-G*, exposures varying from one frame in three fourth seconds to one frame in 356 seconds can be secured. A total of twenty-two change gears is needed to secure this range. Generally speaking, the use of the constant speed drive results in too much or too little footage of film. It is therefore most desirable to provide a wide range of unequal light and dark

intervals. To accomplish this result, one of a series of gears from which a certain number of teeth have been removed, such as gear *X* (called a partial gear) is attached to the far end of Shaft *A*. This gear engages with a pinion (not visible) on the far end of shaft *D*. Shaft *A* is driven by the compound gears at varying rates of speed, depending on the gears chosen. When the teeth of the partial gear (*X*) engage the above mentioned pinion, shaft *B* is turned the proper number of times to secure one camera shutter revolution. During that portion of a revolution when gear *X* opposes to the pinion the portion of its circumference having no teeth, the pinion cannot revolve, with the result that the camera shutter remains either open or shut, depending on the original setting. A small gong automatically operated by a cam on shaft *A*, warns the observer of the closing or opening of the camera shutter as may be preferred. To illustrate—in the device as built a partial gear with nine teeth gives a ratio of light to darkness of nineteen to one, or the reverse as may be desired, or a partial gear with thirty-nine teeth gives a four to one ratio. The gears for the desired ratio are quickly selected from tables which have been computed for the purpose.

The film shown contained the following subjects:

First: A picture of the lunar crater Albategnius taken October 14, 1930, moon's age 21.5 days. In order to increase the scale of the picture a photographic doublet of one inch E.F.L. was used as a magnifying lens, placed so as to give a magnification of 2*x*, the image thus being equivalent to that obtained at primary focus of a telescope of 360 inches focal length. Each frame covers a section of the moon's surface approximately 140 × 215 miles in area. The crater is sixty-four miles in diameter and the central peak is about 5,000 feet in height. The exposure time was one frame in twenty-one seconds and the length of run was 4.5 hours. As the sun sets relative to this place on the moon, one sees the black shadow of the peak in the center of this crater grow longer; creeping across the floor of the crater and then up the opposite

wall. At the same time the shadows of the crater wall nearest the sun grow larger and finally completely cover the central parts of the crater.

Second: A picture of the disc of the planet Jupiter. The intermediate lens is so placed as to produce a magnification of 4*x*. Parts of three nights' work in January and February, 1931 were spliced together to give approximately one rotation of the planet. The exposures range from 13.5 to 17.5 seconds. If attention is fixed on the details of the belts, the rotation of Jupiter is clearly shown. The disc of Jupiter in this film is shown as it would appear to the naked eye. The bay in the South equatorial belt, where the great red spot appeared many years ago, is clearly shown at the left as it is brought into view by the rotation of the planet, and it moves steadily across the picture until it disappears to the right. At about this time a distinct black marking on the North equatorial belt appears at the left and moves across, due to the planet's rotation, until it is a little past the meridian as the picture closes.

Third: A picture of Jupiter and satellites I, II, and IV—taken at primary focus of the telescope March 12, 1931, exposure 13.5 seconds, length of run, four hours and twenty-six minutes. The movement of the satellites in their orbits is very clearly shown. Satellites I and II gradually approach each other until at near conjunction the discs appear to touch. This appearance is due to indifferent seeing.

The work done up to March, 1931 has been largely experimental in a sincere effort to develop a technique as well as an instrument for this purpose, rather than the mere taking of a moving picture of celestial objects.

The first Moon motion pictures were taken during August, 1928. While not satisfactory, they proved that direct lunar motion pictures were feasible and encouraged further effort.

The first pictures of Jupiter were taken October 5, 1930.

NOTE: The experimental film described above proved so satisfactory, that following the presentation of this paper by Dr. Curtis, April 25, 1931, the instrument was changed over to use 35 m.m. film.

INDIVIDUAL VS. MASS STUDIES IN CHILD GROWTH

By C. B. DAVENPORT

(Read April 25, 1931)

About ten years ago T. Brailsford Robertson reported that the course of growth of mammals was not at a uniform rate but that the velocity varied at different ages and that there were typically two extra-uterine spurts of growth. The crest of one of these spurts occurred in the cow at about the fifth month of extra-uterine life and the second at about 20 months. In humans he concluded that there were three spurts of growth, one intra-uterine, a second which he called the juvenile and a third, the adolescent spurt. My own studies on the growth of children in weight were originally made, like Robertson's, on large masses. When the average weights of 100,000 children are taken of different ages, then it is indeed found, as I have reported on various occasions, that the velocity of growth is greatest in intra-uterine life; diminishes regularly (excepting for an extraordinary slump during some days or weeks after birth) until about $3\frac{1}{2}$ years in the male and then slowly climbs up to a maximum at about $14\frac{1}{2}$ years at which time the rate of growth rapidly declines to less than a kilogram a year, beyond 19 years of age. Based upon these mass statistics I drew the conclusion that there were two great spurts in growth; one in intra-uterine life and one, in the case of the male, reaching the maximum at about 14.5 years. The mass of the growth between these maxima can be ascribed to a general body growth involving all of the bodily tissues. The conditions in the girl are like those of the boy, excepting that the adolescent spurt in growth occurs about a year and a half earlier (Figs. 1, 2).

If, instead of weight, we plot the varying rates of growth in stature we get less pronounced maxima. This indicates that

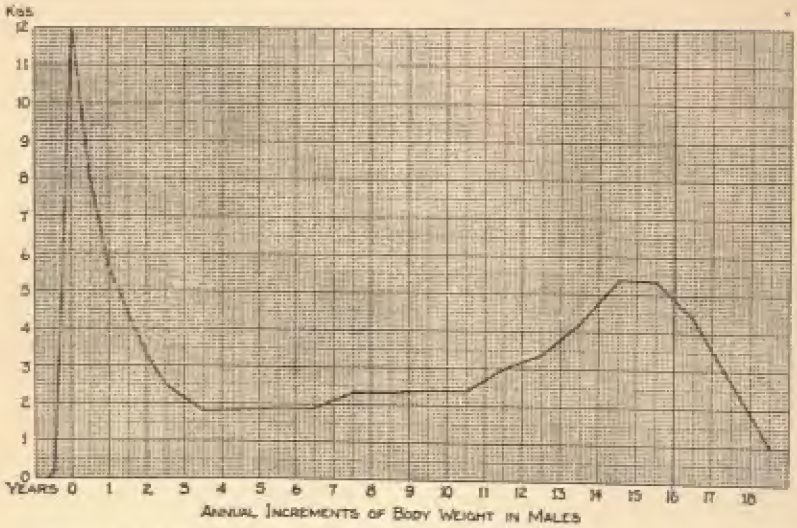


FIG. 1.—The curve of varying average velocity of growth of total body weight, 100,000 males of Nordic stock. Abscissae, age from 9 months before birth to 18 years. Ordinates, rate of increase in body weight per year.

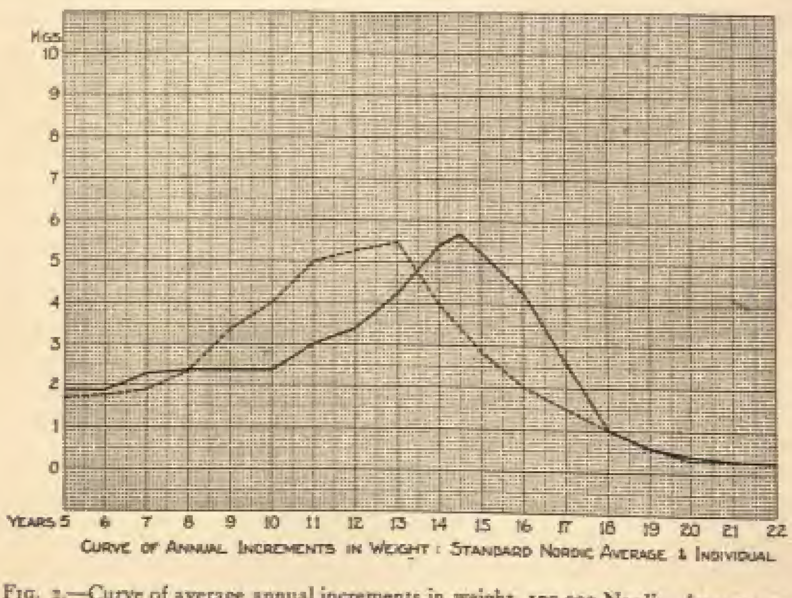


FIG. 2.—Curve of average annual increments in weight, 100,000 Nordics, from 5 to 22 years. Full line, males; dotted line, females.

growth in stature is a more conservative character than in weight [wt. ∝ stat.$^{3 \text{ (or } 2)}$]. Nevertheless the presence of an infantile and of an adolescent spurt of growth is very clear. The crest of the spurt is in the male at 14½ years and in the female at 12½ (Fig. 3).

Robertson interprets these spurts as the bodily manifestation of the activity of a master chemical reaction which, starting slowly, reaches a maximum and then gradually fades

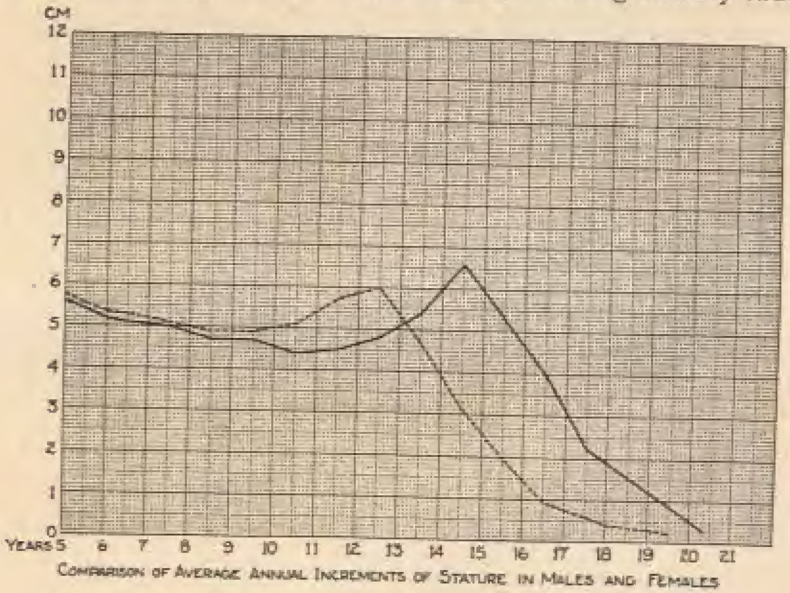


FIG. 3.—Comparison of average annual velocity in stature, male and female.

away. The result is that the curve of development of growth assumes something of the characters of the logistic curve which is found also in the rise and fall of a population. The logistic curve starts with a low velocity; reaches a maximum of velocity and then gradually fades away. Also, in the developmental curve of average stature during the early years the curve is relatively flat; becomes more nearly vertical during adolescence and then becomes nearly horizontal again at the end of the 'teens (Fig. 4).

The master reaction is conceived to be a chemical reaction

occurring in the body of the individual. It is, therefore, important to study the growth, not of the average of 100,000 children but of the individual child. When such a study is

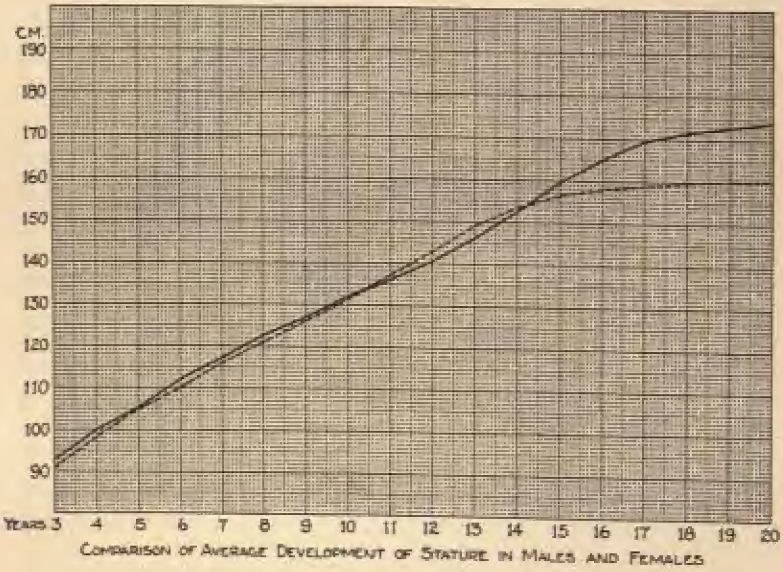


FIG. 4.

made the resulting curve of growth is strikingly different from that of the mass curves. Considering the adolescent spurt, instead of starting gradually to increase in 10 years in boys, reaching a maximum at 14½ and then fading away to 20 years we get in different children a great variation. A very common type is shown in Fig. 5 in which, after a slight decline in weight at 12 years the rapid spurt of growth is delayed until 15 years and then proceeds with almost explosive velocity during a single year, to fade promptly away. The study of different children shows that the age of the maximum spurt is variable. Most commonly it occurs around 14 or 15 years but sometimes much earlier. Another fact is that in some children there are great fluctuations in the velocity of growth in weight during the developmental period even when the child is well; so that there may be two or more spurts of growth in weight.

Weight, however, is subject to such great fluctuations, due to conditions of nutrition and of health that it seems preferable to study the variations in velocity of growth in stature. Here at once the same phenomenon as we have seen in weight is involved, namely that the growth of the individual at adolescence is often of almost explosive velocity as we see in Fig.

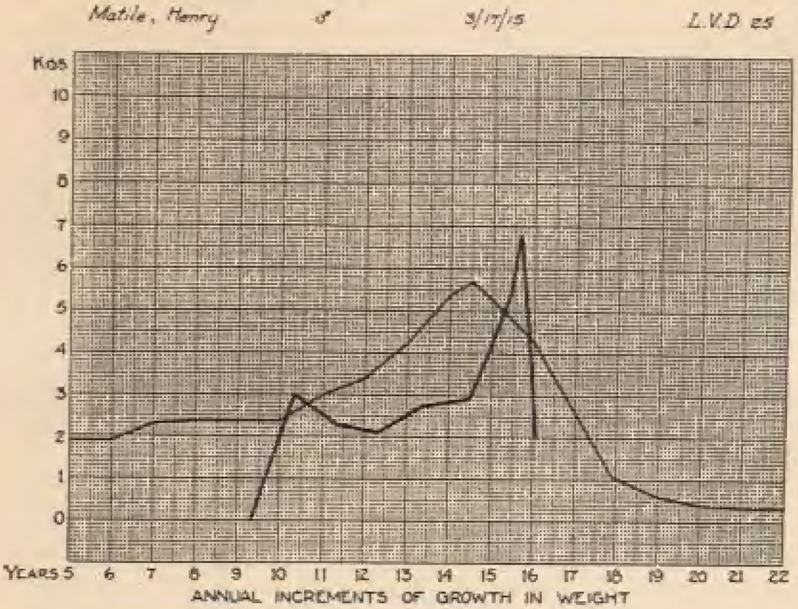


Fig. 5.—Annual increments of growth in weight of a male individual from 9 to 16 years, superimposed on the average increment curve.

6, A. The age at which this spurt occurs is very variable, ranging all of the way from 11 to 17 years (Fig. 7). It is commonly preceded by a period of slow growth in stature, as in Figs. 5 and 6, A. The velocity of growth at the maximum of spurt is also very variable. Sometimes it does not reach over 5 cm. a year (Fig. 6, B). Sometimes it is as much as 15 or more. In some cases again, as in weight, the velocity of growth in stature is very variable (Fig. 9). Also in brothers of the same family some of the curves of varying velocity of growth in stature are apt to be more nearly uniform (Fig. 8).

What light do these curves of individual growth throw

upon the curve of *mass* increment in weight or stature? What is the meaning of this Gaussian curve of distribution in the velocity of growth and weight of masses of children? A study of the individual curves gives the answer. The peak of the mass curve is found at $14\frac{1}{2}$ years because, on the

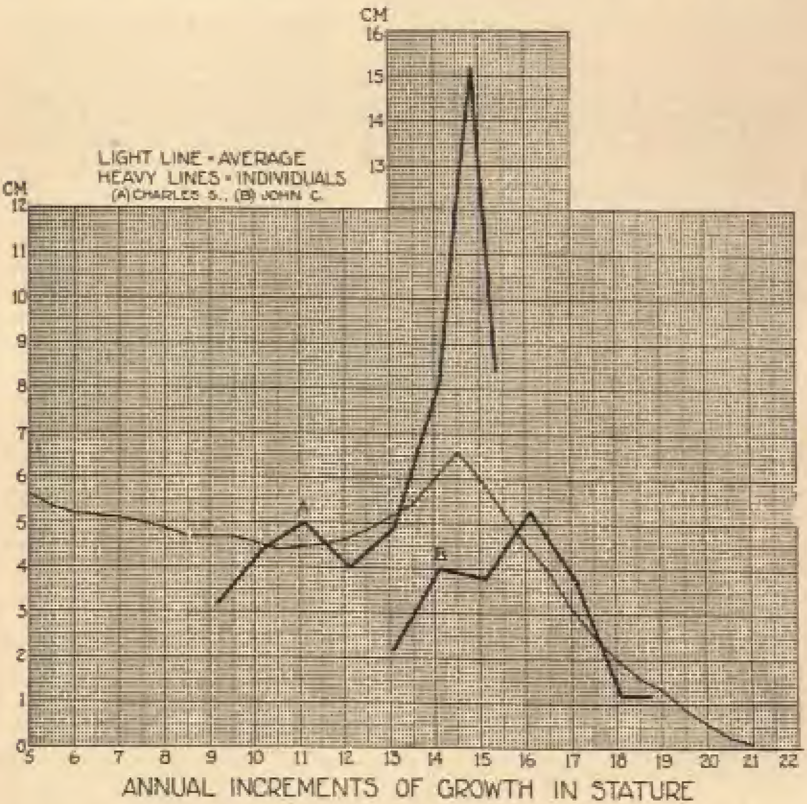


Fig. 6.—Individual increments of growth in stature of 2 boys; *a.* Chas. S.; *b.* John C. 9 (or 13) to 15 or 18½ years.

average the individual spurts of growth are massed around this age. Also the most rapid spurts found in our series have occurred close to this age. *On the average*, the rate of growth of stature of a boy of European stock reaches a maximum at $14\frac{1}{2}$ years. If one departs from this point at either a younger

or an older age the frequency of these spurts diminishes gradually and they are less extreme. The consequence is that the average velocity of growth diminishes above and below $14\frac{1}{2}$ years. The Gaussian curve that results from the mass

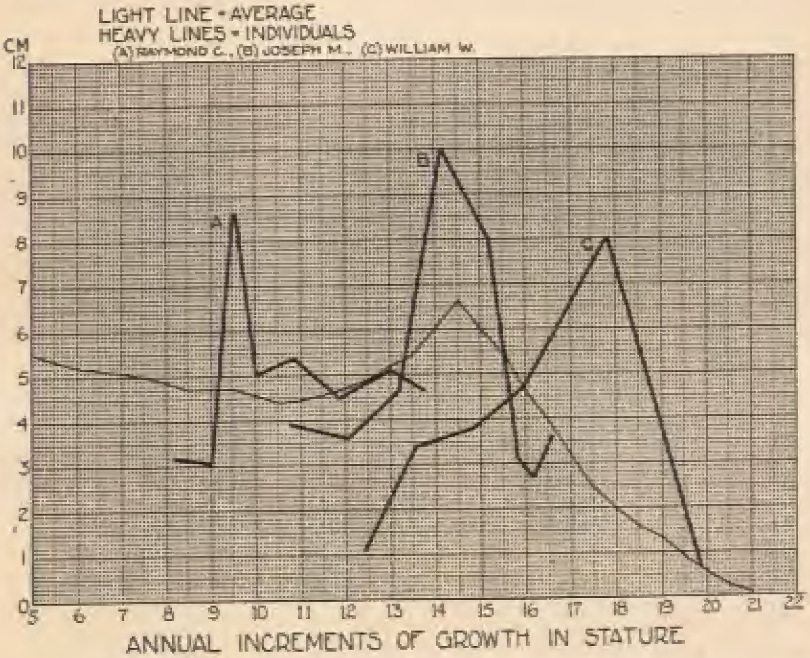


FIG. 7.—Annual increments of growth in stature of 3 Boys, A, B and C indicating variation at age of adolescent spurt.

statistics is then due to the fact that, as in Gaussian curves in general, the mode is at the abscissa of *greatest frequency* of adolescent spurts and these become less frequent the more we depart from the mode. The Gaussian curve, or its complement the logistic curve, gives then no true picture of the varying velocity of growth of the individual and has nothing to do with the form of the development of the chemical master reaction.

The existence of a single mode at 14–15 years is, of course, due to this grouping of the individual modes at that age.

There is, however, in the development of the child often a series of maxima of growth between 6 and 18 years (Fig. 9). Inasmuch as the points of maximum do not occur at the same age or any particular age these minor maxima tend to smooth themselves out in the average curve of growth. If the great

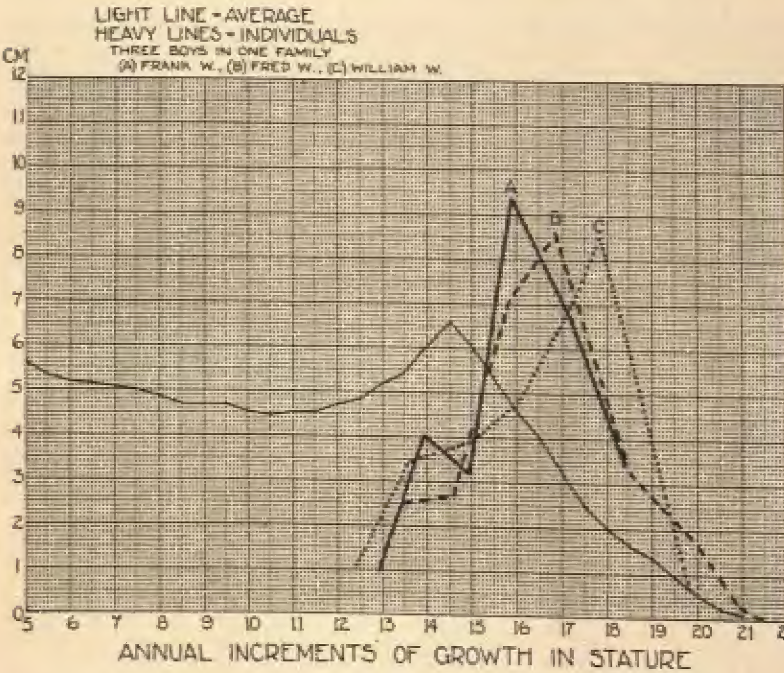


Fig. 8.—Annual increments of growth in stature of 3 brothers, showing similarity in form of spurt, age 15 to 21.

adolescent spurt is due to a master reaction then instead of there being two master reactions in man there is a variable number, in some children only 2; in others 3, 4, 5 or more.

We know little about the significance of the main adolescent spurt. There is reason for believing that it is tied up with the production of hormones from the anterior lobe of the pituitary gland and it is possible that there is a chain of causes in which the activity of the gonads and the thyroid also take their place.

The result of the study of an individual then is to reveal that the growth processes are not so simple as conceived by Robertson but are very complex and the different stimulators of growth come into play at different ages, but these are most active at adolescence.

If a practical application of the foregoing observation is to be made it would seem to lie in this: That the growth of each

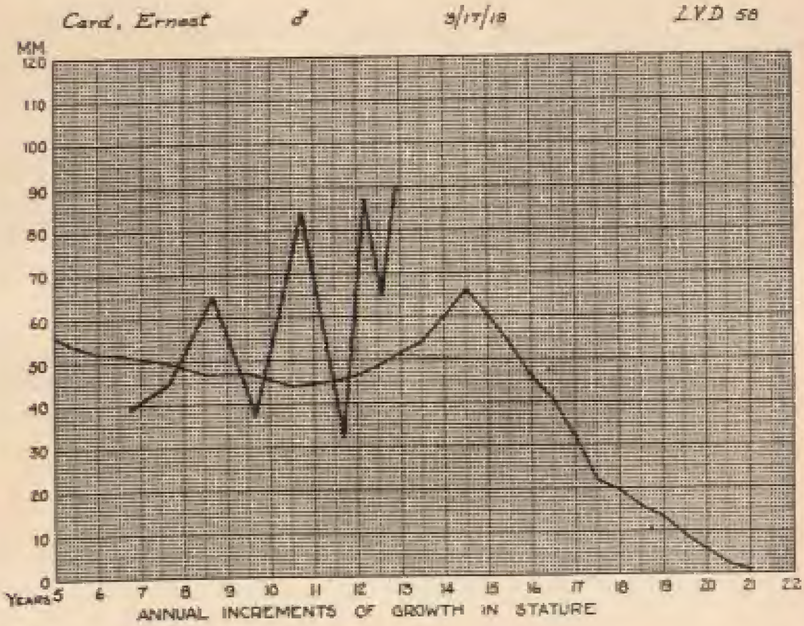


FIG. 9.—Annual increments of growth in stature of a boy, showing fluctuations in velocity of growth in this dimension.

individual child should be determined at frequent intervals, yearly or half yearly, and that during the spurts of growth some allowance should be made for deviations from the ordinary type of activity. For, when a boy is adding 5 or 6 inches in a single year to his stature, it would seem probable that he is in a special chemical state such as may be expected to result in a special type of behavior reaction. At any rate consideration should be given, in the training of a boy or girl, to the physical states that is being passed through.

THE GOTTSCHEE GERMANS OF SLOVENIA

By JOHN DYNELEY PRINCE

(Read by title, April 24, 1931)

VERY little is known of the isolated Germanic minority in Yugoslavia known as the Gottschee Germans who live in a small community not far from Laibach (Ljubljana), entirely surrounded by Slavs.

Strange to say, this small tribe of 15,000 persons is comparatively unknown, even in Yugoslavia. These people attracted attention to themselves early in August of this year by celebrating the 600th anniversary of the foundation of their colony (August 3, 1930). To these ceremonies the German, the Austrian and the American Ministers were invited, an invitation which I unfortunately could not accept, because I was in Czechoslovakia at the time. Being interested, however, in this group, I invited a delegation of Gottscheers to visit me at Bled and was able from them, as well as from a number of wandering peddlers of their race, to learn something of their history, traditions and unique language. The number of these people which is now so small and the fact that they form an island surrounded by Slovenes have made the Yugoslav Government treat them with every consideration, because they are rightly regarded as completely harmless. For centuries, even under Austrian rule, they had little or no connection with other German-speaking people, but were permitted to grow as a thing apart. Their territory, which lies to the southeast of Laibach, in a mountain district, also helps them to preserve their distinctively national character. In 1880, owing to the fact that the small territory allotted them by ancient law was becoming too narrow for the then population of 30,000, 15,000 of them emigrated almost in a body to the United States and Canada, where they and their descendants are still flourishing. Be-

cause there are a great many members of this race in and near Cleveland, Ohio, I was invited as American Minister to the recent celebration. I might add, that I sent them a letter of congratulation in German, together with a poem in their own dialect quoted from almost the only work on the subject, *The Jubilee Festival Book* of their 600th anniversary. They are an extremely prosperous community and owing to the fact of their having been almost unmolested by their Slovene neighbors who look upon them with more or less good-natured contempt, they have carefully preserved their two most distinctive features, namely, their costumes and their almost unintelligible dialect. At the present time, practically every member of the community speaks German and Slovene besides their own language, because the Yugoslav Government, which by the way, sent an official representative to their anniversary last August, has made no effort to check the development either of the religion or the language of the Gottscheers.

The history of the community really dates further back than 600 years, as the first colony from all parts of Austria and even Bavaria and Thuringia came some two centuries earlier. There is a very early church record from this place which dates back to 1177, the time of the Margrave, Albert the Bear, who sent as far as Brandenberg, to obtain German colonists for his territory. This region was dominated by a long line of German-Austrian overlords, the most important of whom, Count Otto von Ortenburg, in 1330, established the district as a regular feudal fief. In 1377, Gottschee attained the official rank of a "market town," and in 1471, the Holy Roman Emperor, Frederick the Fourth, raised the "market" to the rank of a "city" whose seal, given by him, is still preserved in the old city hall. In 1479, the place was made a customs-free district. In the following year it was taken by the Turks who ravaged the entire region and destroyed the even then old church in the town. From 1809 to 1815, Gottschee was a part of the artificial French Kingdom of Illyria, but the inhabitants of this district revolted against the

French rule almost at once and attacked the French garrison in the city. In punishment for this act, the French intended to burn the entire place, but, owing to the petition of the local clergyman, George Jonke, who had rescued many French prisoners from the Gottscheers' fury, the order was rescinded and the place was given over instead to plunder for three days, October 16–18, 1809. Down to the present time, the Gottscheers have retained a marked hatred for anything pertaining to France in contradistinction to the Francophile attitude of their Slovene neighbors and the bulk of the present Yugoslavs.

After the Great War, in 1919, the German-speaking officials and professors were dismissed by the Yugoslav Government, but in 1924, Belgrade relented and gave full voting and language rights to the inhabitants of this region. Of course, in the interim the Gottscheers did not neglect to keep up their local customs and idiom. At the present time, the city and district are supervised by a Slovene financial counselor who is also burgomaster, but the City Council is evenly divided between five Gottschee-Germans and five Slovenes.

The great Apostle of Gottschee, the late Dr. Hans Tschinkel, prepared a grammar of the idiom, in German, but this has not yet been published. Profiting by the speech of such Gottscheers as I have met, and using "Gottscheer Volkslieder" published in 1930 by Walter de Gruyter and Company, of Berlin and Leipzig, I venture to submit the following brief sketch of the language, the first which—so far as I know—has appeared in English.

The language is distinctly a High German phenomenon, belonging to the same group as the Allemannic idioms of Switzerland and Bavaria. Some have compared it to Gothic, owing to its many archaic forms, but Gothic was Low German, using the *t* instead of z (*ts*) in such words as zwei (two), which is universal in Low German, *twee* (Dutch); *två* (Swedish); *to* (Danish); *two* (Eng.), etc.

The Gottschee phonetic system is the same as in German, except that Gottschee *sh* = the French *j* in *jour*.

The following table of changes between modern German and Gottschee will illustrate why the Gottschee is so unintelligible:

German	*Gottschee*	
a	— *o;*	*i konn—ich kann; baas ischt dos—was ist das?*
ā	— *ue;*	*schpuet—spät* (late); *luet—lässt* (leaves).
b	— *p;*	*proait—breit* (wide); *prueder—Bruder* (brother).
d omitted;		*ass—dass; ahin—dahin* (thither).
e	— *a;*	*assen—essen* (eat); *bag—Weg* (road); *labait—lebet* (he lives); *harz—Herz* (heart).
ei	— *oai;*	*hoaiss—heiss* (hot); also *ei—uai*, as *uain—ein* (one).
el	— *au;*	*shauber—selber* (self).
g inserted;		*frage—Frau* (woman); *Du schagest—Du schau(e)st* (thou) seest.
l omitted in *Inlaut;*		*shauber—selber* (self); *hausch—Hals* (neck); *asho—also* (then).
o	— *oa;*	*oar—Ohr* (ear).
ō	— *ea* (*ia*);	*schean* (*schian*)—*schōn* (beautiful).
s	— *sh* (French *j*);	*shein—sein* (to be); *geshund—gesund* (healthy); *shauber—selber* (self).
s	— *sch;*	*ischt—ist* (is).
u	— *a;*	*nar—nur* (only).
w	— *b;*	*bie—wie* (how); *shbester—Schwester* (sister).

The personal pronouns are as follows:

Singular

N.	*i* (*ich*)	*du* (*Du*)
Poss.	*mein dar, mein deu, mein dos*	*dein dar, dein deu, dein dos*
D.	*mir*	*dir*
A.	*mi*	*di*

Plural

N.	*bir, b'r* (*wir*)	*ihr* (*Ihr*)
Poss.	*unsch dar, unsch deu, unsch dos*	*air dar, air deu, air dos*
D.	*insch*	*ai* (= *Euch*)
A.	*insch*	*ai*

Singular

N.	*ar* (*er*)	*shi* (*sie*)	*ins*
Poss.	*shein dar, shein deu, shein dos*	*ihr dar, ihr deu, ihr dos*	*shein dar, shein deu, shein dos*
D.	*ihmon, mon*	*ihr*	*ihmon, mon*
A.	*an, in, 'n*	*shi*	*ins*

Plural

N.	*shei* (*sie* "they")
Poss.	*ihr dar, ihr deu, ihr dos*
D.	*ihnen*
A.	*shei*

The possessives are expressed without inflection of the actual pronoun, gender being indicated by means of the following article: as *mein dar prueder* my brother; *mein deu tufel* my table (*meine Tafel*); *mein dos hausch* my house, etc. Note, however, *unschi atti* our father instead of *unsch dar atti*. When a preposition precedes, the construction is *zam prueder mein* to my brother; *uf shein de knie* upon his knees, etc.

The indefinite article is

	M.	F.	N.
N.	*a* for all three genders		
G.	*ins* or *a*	*a* or *inar*	*ins, es*
D.	*em, im*	*a* or *inar*	*am, im*
A.	*an, in*	*a*	*ins, es*

The definite article which, as just seen, plays so important a rôle in the possessive construction is as follows:

N.	*dar*	*deu*	*dos*	
G.	*des*	*dar*	*des*	
D.	*damon*	*dar*	*damon*	
A.	*dan*	*deu*	*dos*	The feminine *deu* recalls O. H. G. *diu*.

In combination with prepositions, note *zam prueder mein* to my brother; *zan tiefen scheebe* to the deep lake (observe *ze* (*zu*) with the Acc!); *pei damon* = *dabei*, really *bei dem*.

The plural of the definite article is *de*, *dar*, *den*, *de* for all genders.

The relative pronoun is perhaps the most peculiar characteristic formation of the dialect:

M. F. N.
lai ber, *lai be*, *lai bes* = *welcher*, *welche*, *welches*. The A. masculine is *lai bel* = *welchen*. Thus far I have no other forms. Note *dos ringgele lai bes du hon mi gab'n* the ring which you gave to me; *dar bueb lai bel i hon geschaget* the boy whom I saw. The plural is *lai bele*, as *de roaslein lai bele in Valde boksch'n tēnt* the roses which are growing (wachsen thun) in the forest.

The element *lai* is perhaps the Slavic *le* = only, while *ber*, *be*, *bes* are contractions of *welcher* (*b* for *w*), etc.

The noun shows no special grammatical peculiarity except that certain feminine nouns decline in the *n*-declension, as *ze Minein* to Mina. Note the construction: *dem pruedersch de kinder* to the brothers children.

The verb is highly archaic, as will be seen from the following brief citations:

i hon, du hoscht	*ar hot* (*habait*)	*bir hon, ihr habait*
ich habe, Du hast	*er hat*	*wir haben, Ihr habet*

shei hont or *habent* (*sie haben*). The long forms *habait*, *habent* are not used as a rule as auxiliaries (see below).

"Can" is expressed: *i mug*, I can; *bir megen*, we can (= *ich mag-wir mögen*).

i pin	*du pischt*	*ar ischt*	*bir shein*	*ihr sheid*	*shei shein*
ich bin	*Du bist*	*er ist*	*wir sind*	*Ihr seid*	*sie sind*

The imperfect is *ar bart* or *barait* = *er war*, and with the participle *i pin gebán* = ich bin gewesen.

The past participle is expressed both with and without *ge-*, as *i hon geschoait* = *ich habe gesagt*, but *i pin gean* = *ich bin gegangen; shei hont dos et vunn* = *sie haben das nicht gefunden* they have not found that. Note that the negative is always *et*, a contraction for *net* (S. Germ. *nit*). Double negatives are very common, as *pei damon hon i kuain nutzen et* = *dabei habe ich kein Nutzen nicht* I have no use for such a thing. Gottschee shares this peculiarity with Slavonic, as *nikad nisam bio tamo* never have I not been there.

The archaic character of the ordinary 3d person of verbs should be remarked, as *shi baschot = sie wäscht* she is washing; *ar hevot = er hebt* he is lifting; *shei puschont = sie bussen* (*küssen*) they are kissing. In the singular we have also *-ait*, instead of *-ot* as above, as *ar pringait = er bringt* he is bringing; *shi schahait = sie schaut* she is looking. Note also 2d person, plural *ihr geanait = Ihr gehet* you are going.

Of the numerals I have noted *zboai, zbuei = zwei* two; *zbean Herren* to the two gentlemen; neuter plural *zbian roaslein* two roses.

The following phrases will serve to illustrate still further the archaic character of the dialect: *de dirnlein kehrent schmoaraisch schpuet aus* the girls go out early in the morning; *ischt dar prueder dein atinne?* is your brother at home? *dar prueder mein ischt et hoaim* my brother is not at home.

Song

De schpoabar pliehet af Goreisch Hausch	*De Sperber fliegt auf Gregors Haus*
Ber ischt atinne?	*Wer ist darinnen?*
Mina deu junge!	*Mine die junge*
Ber wandelt ummar?	*Wer flieget herüber?*
Matt'l dar junge!	*Matthias der junge*
Roasche, roasche rot	*Rose, Rose rot*
Bos hot Matt'l ze Minein geschoait?	*Was hat Matthias zu Mine gesagt?*

The sparrow-hawk flies over Gregor's house.

Who is within?
Who flies over (me)?
Matthias the young
O rose, red rose
What said Matthias to Mine?

The melodies are simple and the religious songs are very ancient, dating from the mediæval days of the miracle plays.

It is most strange, motoring through Slovenia, suddenly to come upon an ancient oasis which takes one back to pre-

mediæval Germany. Seeing the women's quaint headdresses and hearing this extraordinary language which, in spite of its Germanic character, owing to the odd pronunciation, does not sound in the least like German, the traveller feels almost as if he had been transported to a distant planet whose light-rays show the earth as it was many hundred years ago.

THE AMERICAN LEGATION,
BELGRADE.

LESLIE WILLIAM MILLER

By ARTHUR W. GOODSPEED

Leslie William Miller was born on August 5th, 1848, at Brattleboro, Vermont, the son of Nathan and Hannah (Works) Miller. He received his common school education in Brattleboro, and at the age of thirteen went to work in his father's harness-shop continuing his education unaided, including a good knowledge of Latin through Virgil and Horace. He evinced even at this early age a natural interest in and capacity for art as evidenced by the spirited little drawings that still exist on the margins of his text-books.

He left home on attaining his majority and worked for a time in Orange, Massachusetts, japanning and decorating sewing-machines, and used to tell with great indignation in later years how the proprietor of the shop finding him decorating a machine with a profusion of volutes and flourishes remarked, "Hello, got a new stencil?"

Before long Mr. Miller proceeded to Boston where under the leadership of Walter Smith who had been brought from England, Massachusetts had established its Normal Art School and had introduced drawing into the curriculum of the public schools. He became one of the first pupils of the Normal Art School, from which he graduated and was also a student of the first course in drawing and painting opened at the Museum of Fine Arts in the same city.

He intended to follow the career of a portrait painter, and excellent work of his from this period is still in existence. He engaged also in teaching as a more immediate source of income and before finishing his course of study was already teaching in the Boston schools, and soon afterwards in the Normal School at Salem and the newly-founded Adams Academy at Quincy.

Soon after the Centennial Exposition at Philadelphia in 1876, the School of Industrial Art was established which continued a modest existence for a few years providing limited instruction in instrumental drawing and drawing from the cast. In 1880 the Trustees sent a deputation to Walter Smith in Boston asking him to recommend some one who would organize their School on larger lines and include a definite alliance of art with practical industry, the need for which in this country was perhaps the chief lesson derived from the Centennial Exposition. Mr. Smith said he knew one man, and only one, fitted to undertake the work, and recommended Leslie W. Miller who was forthwith engaged and entered on his new duties in the Fall of 1880.

He found the School at a very low ebb, with but few pupils and no building or equipment of its own, making use of the class-room of the Franklin Institute at such times as the Institute did not use it. From such beginnings the growth under Dr. Miller's guidance was pronounced and steady. Soon it was in quarters of its own on Chestnut street above 17th. A few years later to the dismay of the more timid members of the Board of Trustees, the property at 1336 Spring Garden Street was acquired but in spite of forebodings it soon proved inadequate to house the growing institution. It was enabled however in 1893 to purchase the property at Broad and Pine streets which it still occupies.

Resigning in 1920 after an incumbency of forty years Mr. Miller left behind him a School with a faculty of more than forty members and a student enrollment of some thirteen hundred, an institution which had been a pioneer in its field of Industrial Art in this country and in spite of many followers still held the leading place.

Dr. Miller's interest, however, had not been wholly confined to the School of which he was for all these years the guiding spirit. He was always interested and ready to serve with voice or pen or by labor on committees, in maintaining the importance of Industrial Art Education, of municipal development and improvement, of the extension of the Park

system of Philadelphia, of the regeneration and beautification of the river banks in cities, of the unity of "Civic Beauty and Civic Duty." Through many articles and addresses and much personal labor, of the sort that no-one is aware of until its results are evident, he was for years a moving spirit back of municipal improvements in Philadelphia, and the present Fairmount Parkway, and the improvements of the banks of the Schuylkill now under way are in large measure a monument to his devotion to the cause of civic betterment.

He was a member of the Municipal Art Jury of Philadelphia from its inception until his retirement from active life, at first as Secretary until the organization was perfected, then as Vice-President until his resignation in 1920.

He was a founder of the Art Club of Philadelphis, its Secretary for fourteen years, and its Vice-President for twelve; he was an active member of the Board of Trustees of the Fairmount Park Art Association, and from 1900 to 1920 its Secretary, and in many other associations was actively and practically interested in the welfare of the city of his adoption. He was a member of the American Philosophical Society from 1899, and for many years one of its Curators.

On his retirement in 1920, he was awarded the degree of Doctor of Fine Arts by the University of Pennsylvania, the first award (it is believed) of such degree in this country, and that of Doctor of Laws by Temple University. Withdrawing to what had been for many years his summer home in Oak Bluffs, on the Island of Martha's Vineyard, in Massachusetts, his last years were passed in a calm but not apathetic retirement, and to the last he was alert and interested in the progress of the various educational and public-spirited movements with which his active life had been so closely identified.

He died at Oak Bluffs on March 7th, 1931 at the age of 82 years and seven months.

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